

MIND
A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY

I.—VERBALISTIC TENDENCIES.

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SOME day, let us hope, a better name than Verbalism will be found for the mental attitude, or set of logical habits, which is now commonly and vaguely indicated by that name. Meanwhile we may try to make the notion clearer than it is, and to consider what remedies are possible.

There are two initial difficulties in the way. First, there is no one who proclaims himself a verbalist, and who therefore may be asked to explain the 'ism' from an inside point of view. The name is intentionally dyslogistic and only those who use it as a term of reproach can be expected to give an account of its meaning. And secondly these critics do not suppose that anyone can be a perfectly consistent verbalist. In their view the essence of verbalism is ignorance or disregard of the trickiness of words, and such ignorance or disregard is never more than partial. Complete verbalism would involve an almost impossible departure from common sense. We are all perhaps liable at times to take the so-called Laws of Thought for more than they are worth, but the perfectly consistent verbalist would be one who always interprets and uses them as infallible rules for application wherever predicates are attached to subjects. And though common sense often assumes too readily in particular cases that 'A' is A, or that the predicate A is perfectly distinct from non-A, it can hardly help being aware that so-called A is often wrongly so called, and that the distinction between the predicates A and

non-A is often unsatisfactory. Thus even the commonest common sense uses a certain amount of discrimination in applying the Laws of Thought to actual predications. And even the most consistently formal logician does not openly claim that they are infallible rules for application in particular cases.

We are here thinking of Verbalism, then, as an affair of tendencies only, or of occasional lapses. And since these lapses happen only now and then there is some analogy between Verbalism and certain infectious diseases such as a common cold. The germs of it may be everywhere about us but they only succeed in establishing themselves when they find us receptive to them through a temporary state of weakness. Medical science has something to tell us about predisposing causes of successful infection, and in the same way logical science may throw some light upon the habits of mind that predispose us to fall into Verbalism.

There is, for example, the common habit of confusing two different kinds of interest in verbal statements : the logical interest which is concerned with the avoidance of misunderstandings between disputants, and the grammatical or literary interest which is concerned with the 'correct' or the 'best' expression of given meanings. Both these enquiries are respectable, and both have difficulties of their own to contend with, but we do not help matters by mixing them up. The leading difference between them is that correctness of interpretation is avoidance of error in a matter of fact, while correctness of expression is the avoidance of solecism (or similar defects) in the use of words. On the one side we have the problem of discovering, if possible, what a particular speaker did in fact mean when he said (*e.g.*) that all men are liars, or that Socrates was a sophist ; on the other side the problem is to decide, *when we already know the intended meaning of his statement*, whether the expression he used could have been improved.

It may seem strange that we should ever be tempted to confuse two enquiries so obviously different. But the confusion does frequently occur, and the real importance of 'correctness' in general seems to be the chief excuse for it. Both the correct interpretation of a statement and the correct expression of a meaning do depend partly on our knowledge of the customary usage of words. Our inevitable limitations in regard to such knowledge—for instance in the matter of technical terms—make it difficult to interpret some statements when 'correctly' expressed, and *equally* make us liable to express a meaning incorrectly. The two different enquiries have therefore so much as

this in common ; sheer ignorance of custom may on occasion be corrected by both. Even the great lexicographer himself—we have often been reminded—once confessed to “ignorance, Madam, pure ignorance”.

In our verbalistic moods, however, we are inclined to make too much of this partial resemblance between the two different enquiries. We are then inclined to assume that ignorance of dictionary meanings is the chief (or perhaps the only) difficulty in the problem of *interpretation*. And in virtue of this assumption some people have been led to misunderstand the logic which contends that ignorance of dictionary meanings has hardly any importance for that problem. They then sometimes¹ take this logical contention as implying the absurd doctrine that there is properly no such thing as an incorrect use of words ; that, for instance, no one has any ground for objecting if a speaker chooses to use the words ‘black’ and ‘white’ as synonymous or to transpose their meanings.

If, on the other hand, we keep distinct from each other the grammatical or literary problem of using words correctly and the logical problem of interpreting actual statements correctly we are better able to recognise other sources of misinterpretation than ignorance of verbal custom. A minor one of these is visible even to the verbalist himself, it occurs when the dictionary recognises more than one correct meaning for a given word, so that a statement in which that word occurs may be ‘equivocal’. This is the only kind of ambiguity which the traditional logic was able to recognise, and since it may conceivably now and then occur it is worth a passing notice, though actual examples of it are not easy to find or even to imagine. If such a mistake ever did occur its correction would be quick and easy.

But the really troublesome source of ambiguity is of another kind. It is specially troublesome because it arises from knowledge instead of from ignorance ; from knowledge of facts instead of from ignorance of custom. It is felt only by those who, when disputing a statement, wish to do full justice to a speaker’s intended meaning instead of verbalistically insisting that he ‘must’ mean exactly what he *says*.² On very many occasions of disputed assertions one party thinks that the other has taken too superficial a view ; that he has left out of sight some important details in a fact, or has overlooked some important exceptions to a useful

¹ For a recent example of this common mistake see Mr. Mace’s remarks in *MIND* for January, 1932, p. 211.

² See Schiller’s remarks, in *Logic for Use*, pp. 11, 12, on “meaning what you say”.

rule. Taking his assertion as it verbally stands there is often some uncertainty whether this accusation is just. He may, for excellent reasons, have expressed himself elliptically, being fully aware of the omitted details or exceptions but wishing not to bore or insult his audience by cumbering the statement with them. Proverbs are a prominent example of this convenient manner of speaking, but the practice of using elliptical expressions is one of the commonest and most useful habits of speech. On the other hand, it is always possible that the statement needing interpretation may correspond to a limited or faulty view of the facts.

In this exceedingly common situation the audience have two courses open. The cheap and easy one is that of observing the letter rather than the spirit, and so calling the verbal statement simply false as it stands; and the more intelligent one is that of treating the statement as having no clear meaning until further explanatory information is provided. The discovery of an ambiguity of this kind, then,—we may here for convenience call it “disputers’ ambiguity”—is in essence a request for additional explanation beyond what the mere dictionary can tell us, the object of such request being to avoid condemning the assertion unfairly. And all that is meant by our recognition of the unimportance of dictionary meanings is that *in this situation* they are known to both parties and are therefore irrelevant to the dispute. Disputers’ ambiguity occurs precisely where speaker and audience have the same acquaintance with dictionary meanings, but where the audience finds them not explicit enough for the purpose of estimating the statement’s truth. The request is for *further* information beyond what the dictionary can give.

This unfortunate mixture of our grammatical and logical interests is encouraged also by the common convenient use of the word ‘proposition’. That word, like all others in the dictionary, has a value of its own and can therefore (with proper precautions) be used without any harm. Grammar is naturally interested in cataloguing usual forms of proposition. There are also some contexts in which logic may use the word unambiguously. Any informational sentence whose meaning is equally clear to speaker and audience may be called indifferently either a proposition, a statement, an assertion, or a judgment. Its meaning being taken as clear, logical interest is then free to consider questions about the evidence for its truth. But in our verbalistic moods we fail to keep clear the difference between the logical interest in the strength of alleged evidence and the equally logical interest in avoiding misinterpretation. We are then liable to

dwell too lightly on the problem of meaning and to assume that verbal statements bear their meaning unmistakably on their face. Because they can truly be called 'propositions' and because propositions are taken to be the same as assertions, and because *assertions* devoid of meaning cannot exist, it is supposed that nothing but ignorance of customary forms of speech can justify our finding a given 'proposition' ambiguous. It is obvious that in uncontroversial matter thousands of propositions do bear their meanings unmistakably on their face. When the truth of a statement is accepted without question, difficulties of interpretation are not felt. And even in controversial matters they may on occasion be taken as subordinate to those of evidence. But disputed statements are chiefly those in which precision of meaning becomes important.

The point we are here concerned with is that the uncritical use of the word 'proposition' tends to obscure the important difference between statements which are infected with "disputers' ambiguity" and those which are not. This defect appears absent from propositions—in the traditional sense of the word—because they are not yet actual assertions, but only forms in which assertions may on occasion be made. A proposition as thought of in grammar is a possible form which may be used in some particular context, and so be meant to express an actual assertion. But *when so used* it takes on a "speaker's meaning" in addition to any meaning that belongs to it as a form of words.

A typical example of the confusion between propositions considered as actual assertions and propositions considered merely as words in grammatical combination is provided in Bradley's *Principles of Logic* at p. 145. He there objects to Mill's remark that a proposition may be unmeaning, and claims that this supposed possibility is itself unmeaning. For, he says, "surely on the one hand it is clear that a proposition which has no meaning is no proposition; and surely again on the other hand it is clear that if it does mean anything it is either true or false". If Bradley had understood, or kept in mind, the difference between a 'proposition' in the sense of a verbal sentence and a 'proposition' in the sense of an assertion he might have avoided making so thoughtless an objection to what Mill had said.

This example serves also to illustrate the occasional and temporary character of such verbalistic lapses, since already on the previous page Bradley had shown himself capable of recognising that *questions* may suffer from disputers' ambiguity, and that then there are three alternative answers to these, one of which is that till further explained they are unmeaning. And yet

Bradley's lapses into verbalism were fairly frequent. His insistence on 'meaning what you *say*' has been already referred to (p. 411), and we may also remember his famous blunder in trying to use the Law of Contradiction as an absolute criterion of Ultimate Reality.¹

The habit of ignoring the possibility of a meaningless proposition carries with it the inclination to accept meaningless questions as worth raising. For example the question what electricity really *is*, as distinct from the question how its action may be detected, may be asked in words, but how are we ever to know when we have reached a satisfactory answer to it? Similarly the question what Truth *is*, if supposed to be distinct from the question how we are to recognise a true judgment when we meet with it. The latter question is difficult enough to answer, but the former is devoid of meaning until the latter is already answered. Until we know how to recognise a 'truth' the verbal question as to the Nature of Truth is confessedly out of reach of an answer.

Another verbalistic habit due to the remaining influence of the traditional logic is that of supposing that every *statement* must be either true or false. This can only be maintained of statements which express single (or indivisible) assertions. It may be admitted that any bare unit of assertion—if such a thing exists—must be either true or false; but what security have we that a given statement corresponds to this ideally simple unit of assertion? Wherever two people can agree to take a verbal statement as having an unmistakable meaning—and this is of course a very common experience—they can also agree to regard it as necessarily either true or false. But the moment doubt as to its meaning arises its unitary character vanishes. No longer then have we a statement that must be either wholly true or wholly false, but a statement expressing ambiguously two different assertions, one of which we can perhaps accept as true, and one which we find disputable. The *statement* (or 'proposition') therefore is neither true nor false because it has not yet become a single assertion.

There is also a notable tendency in Proposition-Logic to suppose that the simplicity or singleness of an assertion depends on the simplicity of the sentence used to express it. 'Complex' or 'compound' propositions are then distinguished from simple

¹ *Appearance and Reality*, p. 136. There have been various references to this in other books and articles, but the fullest critical account of it is given in H. V. Knox's book *The Evolution of Truth*, chap. ii., reprinted from *MIND*, April, 1905.

ones by the presence or absence of verbal complications in the form of the statement. Accordingly a proposition like "John is a fool" would pass the test as simple. But regarded as an assertion—regarded as having an intended meaning—its simplicity exists only for unusually simple minds. Common sense, even of medium quality, recognises that there is more than one kind of fool, so that John may be a fool in one respect and not in another, and that pending further explanation of the intended meaning of this 'simple proposition' assent and denial are equally out of place.

This example may help to show us that the meaning intended by a predicate term used in any statement cannot be taken as fully disclosed by the dictionary, but that some of it is only discoverable by raising the question what *follows* from admitting that the statement is true. If such and such a conclusion is supposed to follow we can accept the assertion, while if some other conclusion is indicated we should dispute the asserted fact as wrongly conceived and therefore misleading. The late Prof. De Morgan quotes an instance where some one, in answer to the controversial question "I suppose you will admit that a whole is always greater than its part," says "Not I, until I see what you are going to do with it". De Morgan was himself too much under the influence of Formal Logic to see more in this answer than its paradoxical quality, but we can here get free from his limitations. We are not bound to think only of 'propositions,' but are able to think of statements as made on particular occasions, in a context which throws some additional light upon the meaning intended by the speaker.

It is the context of *Statements* that we are here concerned with. Even verbalism—even the dictionary—does not entirely ignore the context of *words*. Isolated words have a context given to them when they are used in making statements; and the double meanings which even a dictionary can recognise are distinguishable through the different statements into which the isolated word may enter. The word *grave*, for instance, taken by itself, gives us no help in deciding whether its meaning is *tomb* or *serious* or *carve*. We have to wait for this explanation till we find the word used in a statement, which then becomes for a time its context. But this simplest use of the notion of context is the only one open to the old system of logic. The fact that 'propositions' also, when used, have a context which affects their meaning is outside the range of its ideas. And yet the ignoring of such context is one of the chief sources of misunderstanding between opponents.

Admittedly the *complete* context of a statement is beyond

anyone's power to take into account, but that is no reason for avoiding, on principle, all consideration of it, and in ordinary intercourse no one insists on doing so. As we all know, the practice of 'reading between the lines' of a statement is common enough, though often delusive until corrected. Suspicion of hidden meanings is a frequent source of both justified and unjustified opposition and dispute; and that no doubt is some excuse for those who claim that interpretation should always be strictly literal. But what this verbalistic claim overlooks is that the letter and the spirit of a statement deserve equal recognition if we seriously wish to understand a speaker's intended meaning. It is not that in looking for meanings below the merely verbal surface we are taking a needless risk of misinterpretation, but that we always have to choose between two opposite risks—between putting too much and too little meaning into the given statement. In either case there is a risk of misinterpretation to be faced, and our choice between the two risks will naturally depend on the strength of our inclination to do the best we can. After all, if the hidden meaning happens to be there, why should we be forbidden to see it? And though there is never any need to assume without enquiry that such and such a hidden meaning is intended there is often a pressing need to raise this enquiry before deciding whether to accept or to dispute the statement. Anyone who tries to treat an opponent fairly will wish to avoid accusing him of meaning what he has not meant; but the *question* whether he did mean it hurts nobody except the tricky speaker. He alone is inclined to resent it.

The expression 'reading between the lines' has, like many other phrases, a broader and a narrower meaning, the latter being here outside our interest. What we are concerned with is not the provocative inferences which an over-sensitive person may happen to see behind a speaker's words, but the inferences of any kind which are always part of a statement's meaning in its actual context. We must remember that all truths are connected with other truths so that every statement, if it has a meaning, allows of *some* inferences to be drawn from it; we must remember also that the chief value that any piece of information has consists in its influence on our present views by way of extension or correction. And the question whether a given statement is or is not intended to carry with it such and such a consequence is one which a careful interpreter of the *intended* meaning of the statement is always bound to consider. The mere dictionary meaning of the words that are used may help to start this line of enquiry, but it cannot decide the question.

Another common source of verbalistic lapses is the habit of confusing distinctions with differences; *i.e.*, of supposing that distinctions, like differences, are natural facts instead of being artificial instruments used by us to serve our purposes and justified only in relation to such use. We have to remember that distinctions are not differences as such, but are differences when and while they are (rightly or wrongly) supposed to be important in a given context. In our verbalistic moods we are tempted to think of distinctions as either existing or not existing, as either always or never worth recognising, as either true or false. Since simple assertions—*i.e.*, statements when their meaning is taken as clear—must be either true or false, it is carelessly supposed that distinctions also must be reckoned as either sound or unsound generally, instead of being needed for one purpose and irrelevant for another. Here again common sense will usually save us from completely consistent verbalism, since every one recognises that in some instances a distinction drawn between two different kinds of X has a value in one context that it lacks in another. There are, for instance, many distinctions that are obviously relative to context, and therefore have only a limited value; such as rich and poor, hot and cold, old and young. It would need an almost Eleatic verbal pedantry to see in these familiar distinctions anything more than linguistic instruments with a rough and uncertain value in application. But there are other distinctions whose limited instrumental value is less easily seen because the limits of it are seldom met with. There is, for example, the distinction between pure and applied science. In a recent book by Prof. Julian Huxley¹ some of the difficulties arising out of this distinction are noticed, and it is there shown with the help of some examples that the line between 'pure' and 'applied' will not bear application with perfect strictness. Like the distinction between hot and cold it has a certain occasional convenience if we are content to remember its inevitable looseness. But to suppose that there is any 'real' line between these opposites would be futile and misleading. The artificial line, such as it is, suffices for its own rough purposes, and this is the only justification for our use of it.

Closely connected with this is the time-honoured distinction between theoretical and practical truth. Some readers may remember the efforts made by some of our verbalists, about thirty years ago, to take this distinction as fundamental and unavoidable in all possible contexts. The 'pragmatist' view is

¹ *Scientific Research and Social Needs*, p. 404.

that until a verbal statement can be regarded as having an understood meaning we cannot intelligibly ask whether it is true or false, and that until its acceptance as true can be regarded as making a practical difference from its rejection as false we cannot regard it as having a meaning. This was interpreted by the verbalists as implying that the distinction between theory and practice had never any value. They assumed, in short, that because that distinction has a value in many contexts, or for many purposes, therefore its value must remain inviolate throughout. They thus regarded the distinction as a *fact* independent of human purposes, instead of as an occasional purposive recognition of a real difference that may on other occasions be irrelevant and therefore negligible. All that the pragmatists maintained was that the *meaning intended* by any statement is, where doubtful, discoverable only by reference to the practical difference that its acceptance or rejection is supposed by the speaker to make. They were here using the word 'practical' not in contrast with 'theoretical' but in contrast with 'merely verbal,' and so claiming that (in this context) its frequently useful contrast with 'theoretical' became for the time irrelevant and confusing. And it certainly did confuse the verbalists.

Some of this unnecessary confusion might perhaps have been saved if the pragmatists had avoided the use of the word 'practical' to express their meaning. What they had in view was the connection between *verifiability* and meaning—the fact that in the absence of desired information as to a proposed test of the truth of a given statement, that statement—if and when careful criticism is thought desirable—remains for the time incapable of either acceptance or rejection since it lacks definite meaning. Here again the unnoticed difference between a 'proposition' and an actual statement helps to blind us. The 'meaning' of a proposition exists as a rough average taken from many past occasions, while a statement made on one particular occasion may demand a closer inspection due to the context in which it was made. Take, for example, a statement that the Austrian rather than the German Government was the real 'aggressor' in the Great War. Regarded as a mere proposition there is no difficulty about its meaning. An 'aggressor' means, in the dictionary sense, the one who begins a quarrel. Most of us, however, know that the justified application of this word in particular cases is often difficult, and that therefore there is always a risk of its being wrongly applied. In order to guard against this risk, in the particular instance here supposed, we may want to know what test of 'beginning' a war is thought *by the speaker* sufficient.

He professes to rely on facts of a sort, and we wish to make sure—before accepting his statement as true—that he has not conceived his facts too superficially. It is this appeal from words to supposed facts which is meant by the phrase 'justification of a theory by reference to practice'; enquiry into the special circumstances referred to by the theory, with the intention of distinguishing between the relevant and the irrelevant ones, would better describe the method thought desirable.

The custom of avoiding, in Proposition-Logic, all consideration of statements the truth of which is difficult to decide, and where there is much to be said on both sides, is one of the unfortunate logical legacies from past times. Logicians who are still under the influence of the traditional system are often unaware of its hold upon them and believe themselves to have long known and admitted all that is said against it. Yet they continue to use, as their only examples of 'propositions,' statements which are either obviously true or obviously false, neglecting the large intermediate region which includes most of the statements we make or encounter in real life. Logicians of this kind seem to live—for logical purposes—in a world in which blameless people walk about informing each other that 'this grass is green' or that 'some cats are quadrupeds'. Why do they habitually avoid using examples of the real doubts and difficulties of opinion that specially test the value of any logical theory?

The habit of assuming that distinctions are simply the same as differences is one of the obstacles that prevent a clear understanding of the pragmatist logic. It is this habit, for instance, which makes it so difficult for its victims to see that an 'indeterminate flux' of experiences may be void of distinctions without being void of differences.¹ The pragmatist view is not that differences are artificial but that distinctions are so. When distinctions are identified not with differences as such but with differences *supposed to be important* we are free to recognise that a real difference may be rightly ignored in this or that context. The distinction between different races of mankind, for instance, drops out of use when we have something to say about men in general, because in that context the difference has become for the time unimportant; yet the difference itself remains, and may in other contexts be found important. If, for instance, we happen to think that something important is true of pure-bred Germans which is not true of Jews long settled in Germany, we are artificially making a distinction out of a difference which—however

¹ Cf. C. E. M. Joad's *Introduction to Modern Philosophy*, p. 80.

small and however vaguely conceived—is real. Differences, the pragmatists say, exist everywhere, but their importance is relative to the special context in which distinctions are based upon them.

The various references to pragmatism that have here been made will probably seem unsatisfactory to readers who have been falsely told that pragmatism attempts to justify using our wishes as a criterion of truth. This widespread misrepresentation was in its origin innocently based upon William James's attempt to express and defend his real sympathy with some of the wiser followers of the orthodox religion. James seems to have been led in this way to employ the term 'belief' in a more extended meaning than is usual in science or philosophy. Although in speaking of the "Will to Believe" he was careful to explain that he was only referring to questions that "could not be decided on intellectual grounds" he might have made his meaning still clearer if he had used some other word than 'belief' to describe the acceptance of a verbally stated creed without bringing careful criticism to bear upon its meaning. There would have been less harm in calling the latter mental attitude 'faith,' since that name has long been used for the purpose. James might also have made his meaning clearer if he had avoided using the phrase 'intellectual grounds' with its suggestion of some definite contrast between these and other grounds which might deserve to be called emotional. The contrast he probably intended was between a belief grounded on the supposed strength of evidence and the state of mind that despairs of getting evidence for a doctrine which at present remains ambiguous but whose meaning may, we hope, gradually become known to us if we treat it without hostility. To describe such an interim state of mind as belief gives, at first, some excuse for those who, accustomed to seek truth with the help of their full critical powers, imagine that James—and therefore pragmatism—advocates the disuse of critical methods in general. But though this mistake may have been at first excusable when made by hostile readers of James's essay, it has now so often been directly disavowed by pragmatist writers that it has itself become an example of the desire to believe the worst of pragmatism. Its persistence is chiefly due to the efforts of some of our more verbalistic philosophers. Conveniently, if innocently, this has served them as a means of drawing attention away from the attack on Verbalism initiated by Peirce and continued by James and Schiller. Anyhow not one of them has ever yet tried to answer the pragmatism which, while openly condemning desire as a sufficient ground of

belief, regards disputable statements as always liable to unforeseen ambiguity and as always made by some one who tacitly claims, through knowing what he means to assert, the power of removing any ambiguity which his audience has discovered. Verbalists, as such, are shy of the effort to understand what constitutes effective ambiguity.

The foregoing account of some of the leading sources and symptoms of Verbalism is intended also to suggest ways of escaping inoculation by it. In general the method advocated is that of taking more trouble than usual to make sure of the meaning of actual statements before we accept or reject them. For this purpose we have to keep always in mind the real difficulties in the way of doing so. Our method is only needed in cases where we have a strong motive for guarding against misinterpretation; and the strength of this motive naturally varies with the interest we take in the problem presented to us. We do not trouble much to criticise statements of weather forecast, or book reviews, or matters of taste in food. There are also some highly puzzling statements—such as those made by Einstein or Heisenberg—which unmathematical readers leave to others to dispute. But we all frequently encounter statements that rouse our inclination to ask pointed questions before we can accept them as satisfactory.

These are specially the occasions on which we, or our opponents, are liable to slip into verbalism; liable to lose sight of the trickiness of words. The best available general safeguard seems to be a thorough revision of the traditional logical habits of thought. The old logical system is encrusted with survivals from earlier times when the attainment of truth seemed a simpler affair than now. Our 'certainties' are more modest than those of the nineteenth century, and immensely more so than those of three centuries ago. A syllogism has long ceased to be an effective answer to suggested doubts, and has become no more than a needlessly pedantic form in which definite doubts can be displayed and possible errors located. Doubts of the truth of its premisses are now readily put forward, and even the risks of ambiguity in a middle term clamour for recognition.

Looking generally at the sources of the trickiness of language we find them chiefly of two kinds. First there is the fact that descriptive words, as such, must always be indefinite when applied, since any individual case is more full of detail than even the fullest description can take note of. In describing, therefore, we are always compelled to make a choice between the details that seem important and those that do not; and in our choice there is unfortunately always room for error. A special oppor-

tunity for error in applying this distinction is our liability to forget that importance varies with variations of purpose, while purpose varies with the context in which a statement is made on any given occasion. It is easier but less accurate to think of importance as a quality which varies simply in amount, so that a detail's importance—great or small—belongs to the detail itself independently of the occasion to which the statement is relative.

The second main source of the trickiness of language is the fact that all assertion assumes the need of a sharp distinction between yes and no. That is to say, whenever we have interpreted a statement and so have made it into an assertion, we are tempted to apply the Law of Excluded Middle to it, thereby assuming that we have before us a 'single' assertion, not divisible into parts, some of which may be truer than others. Again it is easier but less accurate to overlook the difficulty of imagining an assertion which can safely be taken as indivisible. In order to criticise an assertion fairly instead of by methods suitable to the debating society or the hustings we have to suppose that the assertor (being more or less intelligent) had some excuse for his error—that he saw part of the truth though not enough of it. Intelligent criticism tries to make out exactly how the assertor's mistake arose, or how far we can agree with him, and the point where our view departs from his. Not only our hopes of convincing him but also of enlarging our own view of the facts depends on our patience in using this difficult method of criticism.

These are the two chief obstacles that belong to the inmost nature of language. At present it seems that whether we regard them as defects or not they are part of the price that we have to pay for possessing a language at all. No way of curing them has yet been suggested and so we can only regard them as traps to be avoided if and when we can.

A third important source of trouble belongs rather to the conditions of using language than to its inmost nature. It consists in a speaker's need of striking the best balance between conciseness and fullness of expression. When a statement expresses less than the whole of the speaker's intended meaning the audience have some excuse for neglecting the missing portions. On the other hand if a speaker, with the best intentions of supplying this defect, were to mention all the exceptions he has in view, or to give a full account of the relative importance of the details in his asserted facts, the tax upon the patience of his audience or upon their power of mastering the complexities of his statement would be unbearable. In consequence of this two-sided

risk of being misunderstood a speaker has to make the best compromise he can ; he has to guess at a given audience's power of reading correctly between the lines of his clipped statement. In uncontroversial matter this method usually meets with enough success, but in controversial matter it notoriously often fails. In the traditional logic this difficulty has always been ignored. The notion of translating ordinary statements into 'logical form' naturally tempts the logician to play for safety by giving to any statement the minimum of assertive meaning that it can grammatically bear. In this way a speaker is not allowed to mean what he means, but only what under the strictest verbalistic interpretation he 'says'. In the name of Logic we thus get strict rules about the only permissible meaning of words like *some, or, most, few* ; rules which might have a kind of value in a book on elementary English grammar.

In conclusion : Verbalism has here been regarded as a philosophical disease to which we are liable to succumb in our weaker moments. Outside philosophy there is something to be said for it on the score of rough convenience. The ambiguity of the word 'proposition' is often harmless, and we may on occasion talk of 'literal truth' without ignoring the difference between letter and spirit. Again, the Law of Excluded Middle holds good between two people who are agreed upon the singleness of the assertion intended by a given statement. In casual everyday talk, or between two persons who are content to treat their differences of opinion lightly, or wherever we are not interested in getting as near as possible to the truth, verbalistic habits of thought do no particular harm. The harm done by them is greatest where we are trying—or professing—to perform our reasoning in the best possible manner, as for instance in our philosophy. Philosophical enquiries, more than any other, are liable to be smothered in Verbalism. That is the chief reason why logic should be specially hostile to it. In experimental science the temptation is slight, and in civilised common sense the risks are well known and a discount is made for any resulting errors. But while common sense is free to dispense with any careful study of logic, philosophy cannot decently claim any such freedom. It has to base itself upon a logic of some sort, and it is still too apt to rely upon a system which is innocent of modern difficulties in the search for truth.

II.—THE EXOTERIC APPROACH TO HEGEL'S "PHENOMENOLOGY". (I.)

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IN 1807 Hegel published his first major work, *Die Phaenomenologie des Geistes*, which he described as the "first" part of the system. The sequel to it, contained in the *Logic* and the *Encyclopaedia*, though present to his mind in rudimentary form, he regarded as constituting a separate undertaking presupposing the "deduction" furnished by the Phenomenology. The system must be thus understood in the light of the Phenomenology and not inversely. Two considerations suffice to establish this view. In the first place, Hegel himself wished that the argument of the Phenomenology be judged on its own merits; neither in the text nor in its preface or introduction is there any allusion to the contrary. As far as I am aware, Hegel nowhere made the demand that his first chief publication be understood in terms other than those in which he chose to formulate its contents. In the second place, in the introduction to the *Logic*, we find, quite deliberately, priority conferred upon the Phenomenology, the "deduction" of it being explicitly "assumed" in the new treatise, the theme of which Hegel asserted to be the "system of pure reason" or the "realm of pure thought," a system or realm dominated by the concept of "science" propounded in the earlier work. To fly in the face of Hegel's own procedure by interpreting the Phenomenology in terms characteristic of his subsequent system is a case of putting the cart before the horse.

There is, however, one apparent justification for reading into the treatise of 1807 ideas peculiar to its sequel. It lies in the possibility of regarding the sequel as being a sequel in the order of publication but not in the order of conception. Hegel's philosophic efforts, embodied in miscellaneous products, may be invoked to show that, prior to the Phenomenology, he had already delineated the essential tenets associated with his *Logic* and his *Encyclopaedia*. Aside from the essays printed by himself during his formative period, essays foreshadowing but

little his later doctrine, Hegel left behind him manuscripts of a number of sketches dealing with theological, ethical, and metaphysical subjects, some of which anticipate in outline, if not in detail, his later system of "panlogism". The extent of such anticipation may now be examined for the first time, his early manuscripts having been made available, in the chronological order in which they were penned, by the painstaking labour of competent editors, such as Nohl, Ehrenberg, and Lasson. Among these early writings, not intended by Hegel for publication, is a draft of a system, dealing with logic, metaphysics, and the philosophy of nature, the composition of which antedates that of the *Phenomenology* by several years. This being the case, it would seem legitimate to restore the historical order of Hegel's thought and to regard the argument advanced in the *Phenomenology* as the outcome of ideas anterior to it in formulation.

The interpretation of the *Phenomenology* from the standpoint of the system, the chronology of the *conception* of the system being deemed more important than that of its *publication*, raises certain difficulties that must be met if the proffered interpretation is to be accepted as valid. If the system antedates the *Phenomenology*, why did Hegel deliberately ignore it, setting it aside in favour of the "phenomenological" argument, an argument he himself considered primary, as his own unequivocal words in the introduction to the *Logic* sufficiently testify? To state the argument of the *Phenomenology*, not in its own terms, but in the terms drawn from Hegel's "later" system, just because the philosopher had in his private possession an "early" sketch of that system, is to change the order in which Hegel himself chose to offer to the public the results of his reflections. Whence the justification for altering the author's design in withholding the system until he had completed an investigation affirmed by himself to be logically prior? Only the interest in genesis would seem to justify reversing Hegel's own procedure. Baffled by the nature and the outcome of the "phenomenological" argument, the interpreter, conversant as he is with the method and result of Hegel's entire philosophy, cannot resist the temptation to trace its origin to a unitary conception, the conception of "panlogism," present in all Hegelian products, as the principle of their continuity and development. And since of such a conception we possess a datable record, the main features of the *Logic* and the *Encyclopaedia* having been drawn by Hegel long before 1807, what more natural than to see in the system the *fons et origo* of the

argument presented in the *Phenomenology*? It would indeed be natural so to account for the *Phenomenology*, if the system preceding it had no roots in anything anterior. We are at liberty to regard an original system as the source of all the later products of Hegel's genius if we choose to neglect its relation to ideas entertained by the philosopher earlier. After all, the system was no *causa sui*; it is futile to invoke a background for the *Phenomenology* without enlarging it to include ideas generative of the system itself. Now it happens that among the manuscripts of Hegel, antedating those comprising the primitive system, there is a group, dealing with theological subjects, in which the philosopher adumbrated ideas so totally at variance with his later panlogism that those who believe in the continuity of Hegel's thought are faced with a formidable problem. What is the relation between the collection of essays included in Nohl's edition of *Hegels theologische Jugendschriften*, in which Hegel appears as a fervent mystic, and the sketch of panlogism edited by Ehrenberg under the title *Hegels Erstes System*, neither of which were intended by Hegel for publication, and both of which formed the background for the *Phenomenology*? Did Hegel at one time have a "mystical experience," repudiating it later under the dominance of a new insight? Was his first system but a prolongation and expansion of a mystical vision? Was his mysticism but the deliberate experiment of a reflective mind, viewing the mystic attitude *ab intra*, as it were, for the sake of exhibiting its dialectic? These questions are not impertinent. They suggest the possibility of utilising the essays and sketches Hegel left unpublished in behalf of different hypotheses regarding the genesis of the system.

The two difficulties attending the interpretation of the *Phenomenology* in terms of the system—the difficulty of explaining Hegel's own view of the argument contained in his treatise of 1807 as one *not* presupposing a pre-existing system, and the difficulty of relating the system to earlier ideas inconsistent with it—oblige us to reconstruct the philosopher's mental biography, a delicate matter concerning which certainty is not possible. Attempts in this direction on the part of interpreters would seem to warrant the belief that about Hegel there is a "secret" still undisclosed. Those bent on laying bare the "secret" have, to quote W. Wallace, "contradicted each other, almost as variously as the several commentators on the Bible". Whose "higher criticism" is to be accepted? Should we, following R. Haym, look for the origin of Hegel's "ideology"

in Hellas, the system being a *Kunstwerk des Erkennens*, something factitious in conscious imitation of Greek genius? Should we, guided by Hutchison Stirling, ascribe to Hegel the intention of doing nothing more than to revise and to systematise the work of his predecessors—Kant, Fichte, and Schelling? Should we adopt the opinion of those who, like J. B. Baillie, see in Hegel's pure reason the original source of his philosophy, historical and contemporary ideas being "clues" to it rather than germs thereof? Or should we conclude with Dilthey that, all influences apart, the propulsive force of Hegel's method and doctrine is to be sought in a mystic vision? Whose authority are we to rely upon? But assuming Hegel's mental biography no longer a secret, its application to the nature of the argument propounded in the *Phenomenology* has little relevancy; the finished expression of the argument addressed to the reader is the thing that matters, not the inchoate or embryonic shapes of it growing in the interior of the author's mind. Solely as formed and issued can a philosophic argument solicit criticism or assent; its manner of conception and period of gestation have as a rule nothing to do with the case. This, at all events, is the attitude of every author who desires that the product of his labour be judged on its merits; it does not appear that Hegel thought otherwise.

If we read the *Phenomenology* as its author meant it to be read, reference to its genesis or sequel becomes gratuitous. We must seek to understand its argument as Hegel himself proffered it, namely, as something assumed to carry conviction by its own weight and force. To ask of the reader that he approach the *Phenomenology* with a knowledge of its antecedents and consequences is certainly more than the author himself required. If Hegel had demanded such knowledge he would have supplied it. To go behind or beyond the declared purpose of the work is to adopt the method of psycho-analysis, a method which may or may not be valid, but, concerned as it is with motives either hidden or suppressed, can acquaint us only with Hegel's mentality, not with the logical character of an argument launched by him as something capable of speaking for itself. Why should Hegel be singled out for psycho-analytical treatment? When in 1934 a philosophic work appears embodying an argument concerning the nature and extent and validity of knowledge, the reader is not obliged, in order to understand its import and to assess its worth, to delve into the mental biography of the author, seeking for clues to the "secret" of its origin or *arrière pensée*. There is no reason why the case should

be otherwise with a work published in 1807. The work is the thing. It stands on its own feet. The reader need assume nothing more than what the author in his preface and introduction demands that he assume. Thus to grasp the meaning of the *Phenomenology* is to grasp it in the only way in which Hegel intended to convey it. To ignore his intention is to indulge in psycho-analysis, *i.e.*, to import into the document of 1807 purposes and ideas not intrinsic to it but intrinsic to the mind of the philosopher as revealed in works either prior or posterior thereto.

The exoteric approach to the *Phenomenology* as distinguished from a psycho-analytical one is the approach to it from the standpoint of the reader to whom Hegel submitted his work in 1807. The only aid to its understanding supplied by the author is contained in the preface and introduction ; the analysis of these two preliminary statements will show precisely what the reader is expected—by the author himself and not by his inspired interpreter—to glean from the published document.

The first thing that strikes us about the Preface is Hegel's injunction not to take it without a grain of salt ; for he deems it "superfluous" and "unsuitable" to begin, as authors are in the habit of doing, with a statement of his conclusions, and of the principles underlying them. Forewords, which set forth at the outset the final outcome of a philosophic work and the fundamental ideas by which it is established, are inappropriate ; to show this is in part the purpose of Hegel's own *avant-propos*. And in what lies their inappropriateness ? Hegel offers three main reasons. (a) Philosophic *results* are consummations of a rational process without which they are nothing but dogmatic assurances. (b) Philosophic *principles* announced as principles are equally dogmatic, for not in being principles consists their truth but in their systematic development. (c) Philosophic *truth* pertains to knowledge unfolded as an organic whole of different phases, the process of its unfolding being an integral part of it ; and to knowledge thus conceived Hegel gives the name of "science". Obviously, an inaugural statement of "first" principles and "final" results becomes altogether otiose, since on Hegel's view what is merely prefatory can have no scientific validity.

Yet, throwing consistency to the winds, Hegel proceeds to write the very preface he condemns, acquainting the reader with the purpose of his undertaking, its relation to the age, the drift of its thesis, the mode of its procedure, its distinction from works following "unscientific" methods, all in a language that

takes for granted the theory to be expounded and justified. It is a magnificent piece of pontifical rhetoric, laying down the law for the only true philosophy, and rebuking all those professing a different faith. It is a unique *pronunziamento* both in respect to the ruthlessness of its polemic and the audacity of its asseverations. In the name of "science," of which he proposes to furnish the only tenable conception, Hegel assails his predecessors and contemporaries, stigmatizing them in turn as dreamers, as simpletons, as formalists, as dogmatists. Impugning as naïve or as sterile methods hitherto pursued—namely, the intuitive, the empirical, the analytical, the mathematical—he promises to vindicate a new way of knowing that will transcend the traditional bifurcation of truth and error. The new way of knowing—he calls it dialectical—will reveal conflicting ideas or thoughts not as disjunctions but as conjoined. A view of truth will emerge in radical opposition to the prevailing one. Truth will appear as synoptic and dynamic, each of its phases being at once false and necessary, false in isolation but necessary as determining its progress of evolution. Of such a conception of truth, involving its own opposite as an inseparable aspect of itself, the *Phenomenology* is to offer an exposition and defence, a conception comparable to that of a biological organism whose growth and continuity depend upon different stages, the later supplanting yet completing the earlier. As bud and blossom are related to each other in the total life of the plant, the latter fulfilling the former by superseding it, so, Hegel insists, in the total life of reason, when viewed under the form of "science," are concatenated those ideas or thoughts a mind innocent of dialectic is wont to perceive as contradictions. To facilitate the reader's attempt to comprehend a work so novel (for the uninitiated as difficult as the attempt "to walk on his head") Hegel consents to divulge the outcome of his investigation; the propositions employed to accomplish this, in advance of their "dialectical" proof, are indistinguishable, he concedes, from any other "dogmatic assurances".

These dogmatic assurances, having as yet no claim upon our assent, do not lighten the task of comprehension. The terms in which they are couched are unintelligible apart from the argument which is to justify them. Ordinary thought is unable to use the "ladder" provided for its ascent to the standpoint of "science". The ladder is too slippery; it is constructed with the aid of an occult vocabulary. What is all this talk about *Ansichsein*, *Fürsichsein*, *Anundfürsichsein*, *Sich-*

selbstsetzen, Sichanderswerden? What is the meaning of *Dasein, Wesen, Begriff, Substanz, Subjekt, Negativität*? Hegel manipulates these crucial words with reckless inattention to the fact that by means of them no reader can be initiated into the "new" philosophy; consequently, the statements in which such enigmatic terms figure, if designed to help and not to hinder understanding, can hardly serve as clues to the nature of Hegel's "Voyage of Discovery". The Preface to the *Phenomenology*, admittedly a tissue of assurances, all conveyed through an esoteric vehicle, leaves the reader uninformed and unconvinced.

We may consider briefly three of Hegel's assurances for the sake of showing how little information or conviction they can initially carry.

(a) His view requires, says Hegel, that truth be understood and expressed not as Substance but as Subject. This is a cryptic and dark utterance. What is substance, and what is subject? Hegel does not define these crucial and recondite terms; he describes them in the vocabulary of his philosophy still awaiting justification, a vocabulary so repugnant to common thought that what is offered to it is an explanation of *obscurum per obscurius*. Struggle as it may, ordinary thought is unable to translate in its own idiom the different senses Hegel imputes to "substance" and "subject"; harbouring so many meanings, which seem arbitrary until defended (and in the Preface their defence is precluded), these words produce in the reader a bewilderment stultifying Hegel's intent to facilitate by means of them the comprehension of his task. The fact is that in his dogmatic use of concepts, laden with a philosophy as yet unproved, Hegel is playing with loaded dice; there is no obligation on the part of the reader, who is invited to judge the import and worth of the "new" philosophy, to submit to such an indefensible procedure. He has a perfect right to reject a terminology which is not only opaque but which takes for granted the very theory to be expounded. And so when Hegel lays down the proposition that truth must be grasped and stated not as substance but as subject, we remain uninstructed and unpersuaded; we do not understand the terms of the proposition, and we do not see why the proposition should be true. Why should "substance" be disparaged as immediate, simple, undifferentiated, inert, abstract, unreal? And why should "subject" be conceived as something honorific, the epitome of all that is living and actual and concrete? We have only Hegel's assurance, which we are under no compulsion to share, that one of the two categories is higher and the other lower.

(b) The truth is the whole—this is another of Hegel's famous assurances, quite unexceptionable as a general proposition if its constituent terms are not initially "loaded" with a particular theory. Truth envisaged as somehow "whole," is this not the object of every philosopher's quest? The proposition as it stands is a truism. But Hegel does not announce it as such; with the notions of "truth" and "wholeness" construed in his esoteric way, the truism becomes a paradox. For we are bidden at the outset to take it for granted that truth is not truth without error forming an integral part of it, and that wholeness consists of a hierarchy of errors which, since they are essential in its process, shed thereby their actual erroneousness. The whole truth thus appears, according to Hegel's drastic simile, as "a bacchanalian revel, where every member is intoxicated; and because every member passes away in isolation, the revel is just as much in transparent and pure repose". The "sobriety" of ultimate truth thus depends upon the toxic condition with which every proximate truth is infected; or, to speak without allusion to Hegel's simile, truth as consummatory, as true in the end, presupposes inchoate and transitional stages, and these stages, though essential aspects of the whole truth, are as such not wholly true. Of this internal relation of whole and part, priority pertaining to the whole, the Preface offers, and indeed can offer, no proof whatsoever. We have nothing but a compendious formula of which the implications are set forth in a crotchety manner. Certain paradoxes, for all we know, may be plausible enough, but to exalt as absolutely true what appears as ineluctably paradoxical is to put an intolerable strain upon our credulity. We are at liberty to suspend judgment until Hegel shall have proved his case; there is no reason why we should yield uncritical assent to a view of truth presented as a dogmatic assurance and expressed in a language foreign to ordinary thought.

(c) Knowledge is real, asserts Hegel, only when it acquires the form of science, and science is system. There is no true *Wissen* without systematic *Wissenschaft*. This, too, is an assurance the reader can accept without qualm if he is not required to assume, as does the author, that there is but one adequate conception of science, and that a specific system is the only true embodiment of it. "Science" and "system" are not unfamiliar terms; ordinary thought has no difficulty in appropriating them for sundry products of human intelligence realised by various methods. But, as Hegel remarks, "what is familiar is not on that account necessarily under-

stood". Now it is one thing to attack the "familiarity" of concepts through discernment of the anomalies they conceal, a discernment forming one of the principal tasks of the Phenomenology ; it is quite another to demand that old terms be received as harbingers of new meanings, and new meanings thrust out with such startling suddenness that they seem to the reader "as if shot out of a pistol," to turn against Hegel a phrase he uses in disparagement of views not his own. If familiarity with the notion of science is no clue to its nature, Hegel's strange conception of it, stated in tortuous language, has no lien on our acquiescence, unless the unfamiliar as such be deemed worthy of encomium. Hegel has still to prove that the proper understanding of science is one in which the reality of "subject" is prior to that of "substance," in which the moving soul is the "power of negativity," a power that makes for "organic wholeness," such organic wholeness being the "truth," and such truth being the object of a new "system". At all this the *ipse dixit* contained in the Preface provokes astonishment but not enlightenment. Why should the euphemistic name of "science" be reserved for knowledge amenable exclusively to the dialectical form? And why should the designation of "system" be magisterially denied to intellectual endeavours culminating in different syntheses of knowledge? We see no reason for regarding as a foregone conclusion Hegel's initial equations—the equation of "science" with dialectic, and the equation of "system" with his own philosophic product. There is in the Preface no evidence for the asseveration that true science must exist in the shape wrought out for it by Hegel's special system. And an asseveration it remains without any claim upon our acceptance.

The Preface, then, is for the most part a congeries of esoteric assertions not conducive to facilitate the reader's understanding of the argument which is to follow. That argument, though difficult, is not a riddle. Denial of exoteric access to it is hardly in keeping with Hegel's confident expectation that the "public" would in due time come to recognise the inherent truth of his philosophy. But the "public" must not be asked to put its faith in the occult. No philosopher, however profound, may lay the "public" under the obligation to unravel what it finds enigmatic. And since the Preface is unintelligible, contrary opinions of certain self-appointed "representatives" of the philosophic public notwithstanding, it may be safely laid aside until the argument itself shall have told its long and intricate tale.

Not the Preface but the Introduction affords an exoteric approach to the Phenomenology. The Introduction, though containing much that is dark and dogmatic, is concerned more directly with Hegel's undertaking, disclosing as it does (a) the *object* to be examined, (b) the *method* of examining it, and (c) the *attitude* guiding the examination. Before the first step in Hegel's argument can be taken—and here if anywhere it is *le premier pas qui coûte*—there must be no confusion regarding these three things. As a statement designed to lay the foundation for a difficult argument the Introduction is certainly exemplary.

(a) The object to be investigated, Hegel takes pains to make clear at the outset, is not knowledge viewed *in abstracto* either as instrument through which the real is attained or as medium in which the real is reflected. Whether knowledge is asserted to be an active vehicle or a passive mirror, in either case the dualism between being and knowing is laid down as a fundamental postulate, precipitating at once the question concerning their logical distinction and actual relation, a question that easily leads to distrust of truth because the rôle of knowledge is either magnified or belittled. If we view knowledge as having an active part in the formation of its object, we banish beyond our ken the intrinsic nature of the real; and if knowledge is assumed to possess no power to shape and modify its object, serving merely as a diaphanous medium for it, knowledge becomes an otiose redundancy. Either anomaly, the anomaly of allowing knowledge to tamper with the real or the anomaly of divesting knowledge of all function or operation, renders precarious the quest for truth. But need we face such a dilemma? Why begin with stereotyping knowledge as instrument or medium? Neither alternative can be maintained without a host of theoretical assumptions clamoring for examination. When these assumptions are impugned—and what assumptions respecting knowledge are unassailable?—the epistemologies resting on them become a prey of doubt. The scepticism which certain views of knowledge engender may be always escaped through scepticism of these views. There is no reason, as Hegel says, why distrust of truth should not itself be subjected to distrust. In this cavalier fashion Hegel renders *hors de combat* approaches to the problem of knowledge differing from his own.

Is it possible to begin an examination of knowledge without making assumptions concerning its nature? To this question Hegel gives an affirmative answer. Initial assumptions are nothing but assurances; one assurance, he declares, is as good

as another. Knowledge occurs; if it did not occur we should have nothing to examine. But what this occurrence is, in what manner it appears, under what conditions, and in what shapes, all this is a matter of critical study. We are bidden by the author to lay aside our prepossessions and to take knowledge as a phenomenon, the fact of its appearing being an empirical datum, whose characters and relations a "scientific" investigation, pursued by a method that claims to dispense with assumptions, can alone determine and justify. Prior to such an investigation nothing that we say about knowledge can have any value. This, at all events, is the position Hegel takes in the Introduction. And when, inconsistently with that position, he asserts that "the absolute alone is true or the true is alone the absolute" we may dismiss such an assertion as falling under his own ban of "barren assurances". This is the very sort of *trockenes Versichern* Hegel insists should be eschewed.

The object to be examined, then, is knowledge such as it appears to natural consciousness, and by "natural" Hegel means the same as "unscientific". But knowledge occurring in minds not disciplined by science, Hegel characterises without much ado as unreal. For this the justification seems to lie in nothing more than in the adroit use of a polysynthetic expression. The word *erscheinend*, which Hegel applies to knowledge considered as an event in natural consciousness, implies meanings analogous to those borne by the English term *apparent*. For what is apparent may signify (1) that which appears or (2) that which is self-evident or (3) that which is specious. And precisely these three senses Hegel attaches to "natural" knowledge: he represents it as a phenomenon which always appears as if it were obvious or self-evident, but when so appearing it is unreal, knowledge being genuine or substantial only when supported or substantiated by evidence amenable to reason. By means of a well-chosen epithet Hegel manages to indicate at one fell swoop his inquiry's starting-point, direction, and goal. Apparent knowledge being the theme of the Phenomenology, the treatise will begin with a form of knowledge first in the order of "apparentness"; it will prove the spuriousness of the first appearance of knowledge by undermining its implied claims to certainty, the process of undermining them leading to a second appearance, more inclusive and more sophisticated, which under scrutiny must yield the palm of certainty to a succeeding appearance; the process of showing that higher appearances of knowledge emerge from a

criticism of lower ones will continue until a point is reached where knowledge, as Hegel says, "is no longer compelled to go beyond itself," and such knowledge, representing the synthesis of all previous phenomenal forms, will cease thereby being "apparent" and become real or "scientific". Thus apposite is the compendious term descriptive of the object with which the Phenomenology is concerned.

(b) The method pertinent to the study of knowledge as it flourishes in "unscientific" minds is dictated by the very description of it as apparent. The method, according to Hegel, must be sceptical if apparent knowledge is to cease being apparent in the sense in which its protagonist asserts it to be and is to remain apparent in the sense in which the philosophic critic employs the epithet. The *bewusste Einsicht in die Unwahrheit des erscheinenden Wissens*—this it is which the sceptical method seeks to bring about in the face of the claim that the only true knowledge is apparent knowledge. From the standpoint of those who identify the apparent with the evident or the obvious, the method will seem to be maliciously negative, aiming as it does to induce in "natural" consciousness doubt or despair concerning what such consciousness feels justified in taking for granted; for the "scientific" critic of natural consciousness, for whom the apparent is equivalent to the specious or the spurious, the method will prove a blessing in disguise, since it will show that the road to real knowledge is not straight and narrow but one to which unreal forms are contributory paths. Hegel is careful to note that the scepticism he advocates, though a thorn in the flesh of natural consciousness, is the soul and leaven of scientific consciousness; negating the apparent (*i.e.*, recognising as spurious what at first is posited as evident) is tantamount to affirming the non-apparent (*i.e.*, acknowledging as genuine what flies in the face of ordinary thought). "The exhibition of untrue consciousness in its untruth," as Hegel puts it, "is not merely a negative process". That scepticism which culminates in complete and wholesale negation, being a sort of *cul-de-sac* into which natural consciousness is led by yielding without resistance to the seductive power of doubt, is a transitional form of apparent knowledge to be grasped and overcome in the course of the Phenomenology. The scepticism urged by Hegel is not an impasse but a method; it is a negative *way* constructive thought must follow and not a negative *state* for it to rest in. Such scepticism is specific or qualified (Hegel characterises it as *determinate*); it is addressed to the particular or definite forms of apparent knowledge whose

claims to truth are shown to be false claims ; it must, therefore, not be confused with universal or unqualified scepticism which renders truth itself impossible of attainment. Like a war to end war, Hegel's scepticism aims at its own ultimate destruction.

The sceptical method applied consistently to the successive and connected forms of apparent knowledge, the sequence and concatenation of which are to be determined by the method itself, Hegel speaks of as dialectical. It is a method that builds by destroying or that ravages by constructing. These two aspects are complementary. Real knowledge is not another thing than apparent knowledge ; it is the same knowledge purged of its presumptions and trans-shaped into truth at once continuous and complete. Emphasis on the purgative process alone, ignores the regenerative power which Hegel ascribes to his principle of "negativity". Dialectical castigation of one form of apparent knowledge entails of necessity transition to a new form in which the meaning of the old survives. The dialectical nature of every stage of knowledge signifies both its supersession and rebirth, so that in the entire movement of truth nothing is fixed and nothing is lost. Of this marvellous method, which is able to save the appearances of knowledge by negating them, the treatise is designed to furnish a detailed exposition and justification.

But the most important aspect of the sceptico-positive method has still to be mentioned. Being a method for examining the claims of apparent knowledge some standard is obviously required to guide the examination. How can claims to truth be tested without a valid standard, and what else than a tremendous *petitio* can guarantee its validity ? Either there is no criterion, in which case the testing of knowledge is useless labour ; or the criterion is merely assumed or presupposed, in which case it has but the value of a dogmatic assurance which criticism may perennially challenge. In the light of Hegel's animadversions upon all assumptions and presuppositions, the problem is a formidable one. How solve it ? How begin with a standard, to be recognised as such, without being dogmatically posited ? This difficulty, insurmountable from the standpoint of abstract epistemology, Hegel circumvents by his approach to knowledge as extant and, as it were, already standardised. Instead of testing knowledge in relation to a criterion laid down in advance, a procedure manifestly impossible without assuming the legitimacy of the chosen criterion, Hegel accepts the standard immanent in apparent knowledge, and asks whether with reference to that standard, and to that

standard alone, the alternative is truth or untruth. Dismissing as futile the question whether this or that criterion is abstractly impregnable, Hegel seeks to disengage the tests to which protagonists of apparent knowledge would themselves be willing to submit their claims. This is a procedure which enables Hegel to remain inside the circle of apparent knowledge, assuming nothing save that which such knowledge itself assumes, and testing such knowledge by means of norms indigenous to it and not imported from without. The dialectical method, as Hegel conceives it, is essentially a method of boring from within, and what it bores from within is the complex of knowledge as a phenomenon one of whose constituents is the criterion of its own adequacy.

And what is this criterion which is given, Hegel insists, as soon as knowledge appears on the scene? To answer this question it is necessary to emphasise the unavoidable dualism characteristic of the cognitive situation *qua* cognitive; knowledge involves a relation between two terms, one called "object" and the other the "idea" or "conception" of it. Concerning the *real* nature of the relation, and of the terms, nothing can be said at the outset; at the commencement of the investigation, if Hegel's method be followed, we must address ourselves solely to the cognitive situation as it *appears*. Now it is "natural" consciousness, whose standpoint we are required to adopt, which presents the dualism between "object" and "conception". It is the "knowledge" claimed by such consciousness which manifests itself as the fusion of two elements, one standing for the thing grasped, and the other for the idea grasping it, Hegel's felicitous term for the latter being *Begriff*. The distinction between that which is there to be grasped (the object) and that which grasps it (the conception) is a distinction inherent in knowledge as phenomenon; without that distinction knowledge can neither occur nor persist, and it matters little whether one or the other of the two terms is held to be more "essential," the distinction as indefeasible for natural consciousness being the important fact. But natural consciousness, in claiming to *have* knowledge, claims also that its knowledge is *true*; this is a claim which rests on the affirmation that between the two distinguished terms the relation is one of concordance or congruity. Here is the standard which, according to Hegel, natural consciousness itself invokes for testing the adequacy of its presumptive knowledge. Concordance or congruity (or correspondence, if that word be used in a neutral sense and not in the sense preëmpted for a special theory of truth) between the

object of knowledge and the conception which refers to it—this is a criterion not superimposed upon the cognitive situation from without but one latent in every appearance of it. For the claim to true knowledge, whenever and however made, is nothing else than the claim that a certain conception is appropriate to its object. If it could be shown that the conception demands a different object or the object a different conception, the original claim would collapse, resulting in the emergence of a new situation in which both object and conception would appear in a revised form. This is precisely what dialectical criticism is intent upon doing ; it seeks to effect in natural consciousness, in accordance with its own standard of congruity, a continual change of position with respect to the content and form of knowledge. Given the phenomenon called knowledge, which is a conscious concatenation of two terms proffered as inalienable because of their alleged mutual adequacy, all the dialectical method has to do is to alienate the two terms from each other by showing that the reciprocal congruity claimed for them is in fact specious or spurious. The test by the dialectical method is thus not extraneous ; it consists in examining the allegation of compatibility between object and conception, the two factors of which the cognitive situation is the resultant, an allegation coeval with the appearance of knowledge as its own measure of truth.

(c) A certain attitude, however, is exacted for the application of the dialectical method, demanding careful consideration, since the essence of Hegel's argument chiefly depends upon it. The Introduction does scant justice to a subject of such central importance ; Hegel phrases the attitude too tersely and in the terms of the very argument in behalf of which it is to be assumed. Nevertheless, what Hegel urges, when rendered in the language of ordinary thought, is not at all difficult to grasp. The attitude requisite for dialectical criticism and construction, Hegel contends, is a dual one, involving intellectual *sympathy* and intellectual *detachment*. For only an attitude both sympathetic and detached can impel us to lay aside our prepossessions for the sake of discovering what is indigenous in any appearance soliciting inquiry. Our own fancies and thoughts must be kept at bay. True impartiality means capacity to view a thing at once *ab intra* and *ab extra*—capacity to enter into its interior in order to seize its unique states and movements, and capacity to occupy an external station of survey in order to note the conditions and consequences of its behaviour. To see a thing wholly from within is to neglect the aspect it wears to

a disinterested observer; but to observe it entirely from without is to miss the individuality or specificity which it reveals to intimate experience. Impartiality thus conceived calls for the rhythmic alternation of *appreciation* and *observation*, the first bent upon the inwardness of a thing, and the second upon watching its mode of functioning. A consciousness incapable of such alternation is precluded from pursuing the dialectical method.

So important for an understanding of Hegel's method is the alternation between acquaintance *ab intra* and contemplation *ab extra* that a statement of it in different terms may not be amiss. "Appreciation" and "observation," being words of shifting senses, are, perhaps, not conducive to bring out the full force of Hegel's contention. If we describe the attitude urged by Hegel as experimental, we suggest by a single word the alternation in question, provided we enlarge its meaning to permit its application to products of the mind, such as the Germans felicitously epitomise under the name of *Gedankenexperimente*. Thought may certainly subject to trial a specific work of thought. We do this whenever we attend to an argument proffered as valid. Before pronouncing upon its validity, we must first *attend* to it, which means yielding to its standpoint, adhering to its conditions, reproducing its terms, in short, making it for the time being our own; and, having seized the argument from within, we cannot accept it as valid, if surveying it from without, we detect its want of compatibility either with itself or with the "facts" it leans upon. The invitation to heed any argument is an invitation to perform an intellectual experiment, involving alternation of two points of view, the alternation of appreciative insight and critical vigilance. An experimental attitude of this general sort, which stipulates that no thought can be divulged as defective unless we first identify ourselves with it, or that a thought must be re-created in its native context before we are entitled to challenge it, Hegel requires that we assume before embarking upon the argument of the *Phenomenology*.

Indeed, without such an attitude initially assumed, the *investigation* of the phenomenon called knowledge would either be precluded or it would be precipitated and directed by "barren assurances" subject to continual rebuff. One form of dogmatism rebuking another may in turn be impugned by a third, the truth of each being in the end a matter of faith or caprice. To all that Hegel's brand of experimentalism purports to be the antidote. The "phenomenological" examination of knowledge, guided and controlled by the method of trial, claims to eschew biased assumptions buttressing one-sided tenets. The "dia-

lectical" view of knowledge Hegel deduces, not from "first principles" considered inviolable, but from a series of *Gedanken-experimente* sanctioned by experience. The argument of the Phenomenology, as Hegel gradually unwinds it, is so intimately bound up with a continuous thread of reflective experiments that the method establishing it might with perfect propriety be called experimental rather than dialectical. Both adjectives are virtually interchangeable, describing as they do the same procedure. But this can be shown only by a detailed analysis of the text. At present we are concerned chiefly with the approach to it as indicated for the guidance of the reader in the Introduction. The key to that approach lies in the subject-matter, defined as apparent knowledge, and in the injunction not to import into it our own assumptions or prejudices. We are required in seemingly good faith to accept at their face value diverse forms of knowledge, such as they appear in the course of human experience, and in the tenacious way in which their adherents cling to them; the consequent censure and revision of these appearances of knowledge is induced by the discovery, made in the process of testing them by their own standard, that their claim to truth is but apparent, in the sense of being illusory. Thus crucial is the rôle of the experimenter: in the laboratory of his mind, as it were, he must reconstruct a specific phenomenon in order to watch its mode of behaviour.

The rôle of the experimenter needs to be specified further, since the essence of the "phenomenological" argument hinges upon it. That rôle, a dual one, is exemplified (α) in the reconstruction of the phenomenon to be observed, and (β) in the observation of the phenomenon thus reconstructed. *How* is the phenomenon to be reconstructed, and *what* are we to discern in it? To answer this we must have recourse to a new set of terms which the sequel will amply justify.

(α) The manner of reconstructing the phenomenon, the phenomenon in each case being a unique way of knowing, analogous as it is to the actor's art of impersonation, may not inaptly be described as *histrionic*. There is no impropriety in the description; for what is the art of impersonation if not the art of entering *ab intra* into a character foreign to the impersonator's? The actor must be able to hide his real face behind an alien mask. He must play a part. He must mimic with fidelity the appearance of another being, "suiting the action to the word and the word to the action". He cannot do this without identifying himself for the moment with the inner life of the character he is portraying. And the greater the disparity between his true

and his assumed character, as is for instance the impersonation of Hamlet by a man of sanguine temper and resolute will, the more superb the art. In the illusion of identity between himself and his rôle lies the test of the actor's performance, an illusion only dispelled when, after the play, he appears without his mask to receive the plaudits for his masterly simulation. Such illusion of identity is one aspect of Hegel's method; the investigator must enact in succession the ways of knowing he is examining, he must reproduce their indigenous features, he must rehearse their essential characteristics, without intrusion of his own predilections or preferences. Throughout the course of the *Phenomenology*, Hegel makes much of the distinction between what a thing is *für uns* and what it is *für sich*. What it is "for us," the external observers and critics, is obviously not the measure of its intrinsic nature. Before passing judgment we must first discover what it is "for itself"; relevance of *our* judgment exacts as prerequisite provisional coincidence with *its* unique standpoint. Suppose, for example, it is empiricism we are intent upon examining. The outsider's conception of this type of cognition is emphatically not the true version of it. The only adequate notion of empiricism is the empiricist's. To view things as he does, we must see them through his eyes, react to them in his ways, suit our words to his meanings, and adapt our meanings to his concepts. Without inserting ourselves by an effort of imagination in the mentality of the empiricist we are unable to understand him as he appears to himself. And this is precisely what Hegel endeavours to do. He delineates empiricism (with the impersonation of which the *Phenomenology* begins) in a style consistent with its character, recreating it as it is "for itself," the identity between his point of view and that of his *dramatis persona* being simulated until the histrionic effort leads to the discernment that the type of cognition enacted is inherently incongruous. What is true of empiricism is true of every other subsequent way of knowing. Each to be understood must be impersonated, and only as histrionically reproduced can its inward discrepancy be made manifest.

(β) What *we* are called upon to do once a type of cognition is properly enacted is thus clearly indicated: we are to render palpable the inconsistency or the contradiction it harbours. We can accomplish this, as already remarked, by adopting the very standard which the type in question avows. The standard being congruity between its "object" and its "conception," our task as observers is to show the extent to which the professed standard is honoured in the breach. But the accuracy of the

observation depends upon the faithfulness of the impersonation; no type of cognition may be deemed incongruous unless it is seen to violate its own acknowledged standard. Thus bound to each other are the two aspects of the dialectical method: we are required to enact situations we expect to find discordant. Whether impersonation and observation are not related in circular fashion, our histrionic feats being pre-determined by what they are to reveal when consummated, is a pertinent question. It can be answered only after a detailed examination of Hegel's treatise. As foreshadowed in the Introduction, however, no doubt is left in the reader's mind that the intent of the dialectical method is to impersonate the incongruous. The word which fitly describes such intent is *comic*. The aim of comic art is to render ridiculous situations or characters that are out of joint. The ridiculousness lies in the fact that the protagonists in Comedy run riot in what the spectator perceives clearly to be absurdities. These protagonists suffer from the illusion of perspective; they move in concentric circles; they are imbued with an exaggerated sense of their importance. They are limited, pigheaded, partisan, fatuous. Comedy finds its inexhaustible material in the protean shapes of human folly. And folly is violent straining of the particular—a particular passion, a particular interest, a particular belief, a particular attitude, a particular ideal. In uncovering without scruple the incongruity of everything steeped in its own inordinate particularity, Comedy, in George Meredith's phrase, "is the ultimate civiliser"; for she it is, as this panegyrist of the Comic Spirit puts it, "who proposes the correcting of pretentiousness, of inflation, of dulness, and of the vestiges of rawness and grossness to be found among us". In its sweeping iconoclasm consists the significance of Comedy when deeply conceived; it exposes the *idola mentis* actuating human thought and action, not with the malicious intent of engendering doubt or despair, but for the sake of keeping the world from being self-deluded and hoodwinked. Of such iconoclasm the dialectical method is the directive force, its aim being to represent as awry modes of cognition put forward as exclusive or privileged. But the logical absurdity inherent in particular or prerogative claims to truth, when made manifest by faithful impersonation, furnishes its own logical catharsis. For the recognition of a self-contradictory situation must lead to its abandonment. This postulate of rationalism, governing the construction of all genuine comedy, dominates Hegel's Phenomenology. The Phenomenology is a comedy of errors ingeniously enacted in

the interest of truth. And in Hegel's comedy, too, the aberrations and delusions exposed are visible, not to those ailing from them, but only to those watching them as spectators. Comic figures, not perceiving what goes on "behind their backs," to use Hegel's expression, are unconscious of their own folly. Their comedy is just this, that the incongruity of their claims, so transparent to us, is not apparent to them. Successive impersonation of types of knowledge as comic, comic because they betray to "us" the contradictions hidden from their adherents, this is the task which Hegel assigns to the dialectical method.

The dialectical movement in its alternation of histrionic reproduction and comic observation Hegel equates with experience. The term is misleading if taken in an unqualified sense. That all experience is nothing but a process of rational transition and growth, in which "objects" and "conceptions" become progressively modified in accordance with the method of trial and supersession, this is either an assumption or a deduction. If the former, it is worth nothing; Hegel himself condemns every assumption as an assurance without intrinsic merit. If it is a deduction, it has no force until consummated, the initial statement of it being indistinguishable from an assumption. If Hegel's method is to be pursued, any advance information about experience as a whole, his own included, is gratuitous and impertinent. What Hegel must mean by making "experience" synonymous with "dialectic" is the unique experience of the investigator. The consciousness that examines apparent knowledge experiences it in alternate ways. Hegel distinguishes the dialectic of consciousness by the fact that the object confronting it virtually consists of *two* objects, one being *an sich* and the other *für es*, the former enjoying a nature of its own and the latter having a nature conformable to the "conception" consciousness forms of it. The alleged "truth" of consciousness lies in the claim that between the two natures, the real and the conceived, the relation is that of congruity or correspondence. And it is just this claim we are called upon to show as unwarranted by impersonating every type of consciousness that makes the claim. It is not the consciousness asseverating its own truth whose experience is dialectical, since it is unaware of the incongruity between the two natures of its object, an incongruity detected by us "behind its back," as it were; it is our consciousness that becomes dialectical in the process of "experimenting" with its untenable asseveration. Only upon a singular mode of experimentation, that embodied

in the impersonation of types of consciousness ignorant of their inherent fallaciousness, can a new kind of experience supervene, an experience in which the "power of negativity" is supreme, in which scepticism is the road to knowledge and comedy the life of reason. The consciousness of the experimenter, if he proceeds in the manner proposed by Hegel, must thus be clearly distinguished from the consciousness experimented with; the latter is blissfully unconscious of cutting a comic figure in the eyes of the former. The experimenter's experience differs from that of the experimentee by the additional awareness that his "subject" is self-deceived, and that self-deception is both inevitable and curable. Only an experience characterised by such awareness may be spoken of as dialectical, an experience clearly reserved for the "subject" conducting the experiment but one emphatically denied the "subject" submitted to it. The theory, even if finally demonstrable, that an "absolute" Spirit expresses itself in all the incongruous forms of "natural" consciousness, and that the "dialectical" exposition of these forms in the Phenomenology constitutes a replica of the experience of a super-human mind, is not relevant for the purpose of setting in motion Hegel's experiment. That experiment, whatever its outcome, is initially Hegel's and not God's, and it is Hegel who lays down the conditions for its proper execution. These conditions, to reiterate, are histrionic intuition and comic analysis. The mind conforming to them must be alternately "actor" and "spectator". Nothing else than this alternation renders the supervening experience "dialectical". Such experience occurs nowhere but in a consciousness able to make the experiment in the way Hegel prescribes.

The exoteric approach to Hegel's treatise has now been sufficiently indicated to enable us to enter upon the argument it embodies. The argument rests upon a definite experiment with an intricate subject-matter. That subject-matter is knowledge considered as "phenomenon," and the experiment consists in "impersonating" its successive forms with the intent of showing that the nature of each is indefeasibly "comic". What will ensue from the comic portrayal of the series of cognitive "appearances" cannot be stated in advance. We must perform the experiment, on Hegel's own terms, before we can ascertain the character and the validity of the result. Everything depends upon the success of the experiment. Its failure would leave us either with a *non sequitur* or with a *petitio principii*. The only way to impugn the conclusion is to undermine its experimental foundation. And since the experiment with the

"appearances" of knowledge has a dual direction, demanding faithful impersonation and comic discernment, it is subject to a double test. Is the impersonation authentic, and is the discernment unerring? Faulty intuition and analysis would vitiate an undertaking preëmpting the name of "science"; for the "necessity" imputed to the dialectal method would then disappear. But until the experiment is made we know nothing of its success or failure. The reader is asked by Hegel to lay aside his fancies and thoughts in order to embark upon a new "Voyage of Discovery". The destination, if known to Hegel, the reader cannot see, not even through a glass darkly. The Preface outlines the land of promise but in a manner intelligible only to its inhabitants. It is no doubt a land flowing with milk and honey, but the reader cannot become aware of this until he arrives there. The Introduction, less intent upon the strange city and more upon the way that leads to it, indicates how the start is to be made and under what conditions. Let us by all means set sail on the sea charted by Hegel, not knowing our journey's end. We may suffer complete shipwreck. We may reach the antipodes where everything is topsy-turvy. Or what we shall find may indeed be the city of God. Let the wind blow where it listeth. To enter thus upon Hegel's experiment, allowing neither his bias nor our own to influence the issue, is to proceed in exoteric fashion.

(To be concluded.)

III.—THE CHRISTIAN DOCTRINE OF CREATION AND THE RISE OF MODERN NATURAL SCIENCE.¹

BY M. B. FOSTER.

FOR the convenience of this article I shall use the term "modern science" in a restricted sense, so as to exclude from consideration its most recent developments. Thus by "modern physics" I mean what is now sometimes called "the classical physics". I do this not because I wish to imply that what I say of it is not true also of the most recent developments of science, but because I do not wish to raise the question here whether it is or not.

I approach my subject by way of a consideration of modern philosophy, and I apply the term "modern" to philosophy with a similar restriction, meaning by "modern philosophy" the philosophy which arose at the end of the Middle Ages and developed along the two main lines of Empiricism and Rationalism from Hobbes to Hume and from Descartes to Leibniz. About this philosophy as a whole I shall make two assumptions which I think will not be disputed. The first is that it was devoted (in so far as it was concerned with a theory of nature) mainly to establishing the possibility or justifying the presuppositions of the modern science of nature. It is necessary to use these two alternative descriptions because the relation of philosophy to science varied according to the degree of development which the latter had achieved at the time. By the later centuries of the modern era the sciences of nature had become so firmly established that they formed a datum from which philosophical speculation could start. This does not of course mean that the philosopher dogmatically accepted the truth of any scientific hypothesis.

¹ I have anticipated something of what is said in this article in a paper entitled "The opposition between Hegel and the philosophy of Empiricism" which was read at the third Congress of the International Hegel Society at Rome, Easter, 1933, and published among the proceedings of the Congress. The subjects of the two papers are different enough to be largely complementary, but where they overlap I have not hesitated to repeat my arguments.

He assumed only that a science of nature was possible (because it was actual), enquired into the presuppositions of its possibility, and tested his conclusions by their compatibility with it. This procedure is what Kant first named the Critical Method, but it was to some extent unconsciously anticipated by his predecessors in the later portion of the period which we are considering. But the assertions made about nature by the earlier philosophers of this period, while the sciences of nature were still in the founding, could not be grounded by such a method. Obviously the argument that nature must be such and such because otherwise the science of nature would not be possible, is cogent only when it is granted that the science of nature is actual. What I wish admitted is simply that these pioneers of modern philosophy, writing before the modern science of nature was fully established and not grounding their conclusions on its existence, did yet ascribe to the world of nature those very characteristics which the modern science of nature must presuppose in it as the condition of its own possibility. Thus Descartes, for example, denied that final causes are operative in nature; and modern physics was based upon the presupposition that final causes are not operative in nature. Locke declared that the Real Essence of natural objects was unknowable; and the modern empirical sciences of nature presupposed that the real essence of their objects was unknowable.¹ In a word: the early modern philosophers ascribed to nature the character which constituted it a possible object of modern natural science in advance of the actual establishment of that science.

I wish it admitted, secondly, that, these modern doctrines of nature being, as they were felt by their authors to be, incompatible with the Aristotelian doctrine of nature maintained in the Scholastic philosophy, precisely the element in them which is alien to Aristotle is the ground of the peculiar characteristics by which modern natural science is distinguished from the science of the Greeks or the Scholastics. Thus, to take the same two examples, when Locke asserts that the real essence of natural objects is unknowable, he is both contradicting Aristotle and by the same assertion ascribing to nature the characteristic which necessitates in the science of it that empirical quality by which the modern inductive sciences are distinguished from any science which had preceded them. When Descartes declares that only efficient causes operate in nature, he is substituting for the Aristotelian conception of nature another incompatible with it; and

¹ Because if it were knowable, properties of the object would be deducible from it, not established by the evidence of experience.

the difference between the Cartesian and the Aristotelian conceptions of nature is the ground of the difference between the modern science of physics and its ancient counterpart.

The general question arises: What is the source of the un-Greek elements which were imported into philosophy by the post-Reformation philosophers, and which constitute the modernity of modern philosophy? And the particular question—which is merely part of the general question repeated: What is the source of those un-Greek elements in the modern theory of nature by which the peculiar character of the modern science of nature was to be determined? The answer to the first question is: The Christian revelation, and the answer to the second: The Christian doctrine of creation. The main object of this article is limited to establishing the answer to the particular question, but I will preface the attempt by a few remarks upon the general one.

Opposition to Greek philosophy in general, and to that of Aristotle in particular, was not raised for the first time in history when the post-Reformation philosophers rejected Scholasticism. On the contrary, the opposition between Christian revelation and Greek philosophy was as old as Christianity itself, and the endeavour to overcome it through the progressive assimilation of Christian dogmas by the philosophical understanding was the spring of the whole development of medieval philosophy.¹ Scholasticism itself is much more than a re-edition of Aristotle. If we ask from what source this *plus* is derived, there can be only one answer: it is clearly and obviously derived from the Christian revelation. My contention is that the conflict waged against Aristotle after the Reformation was only a continuation of the conflict waged against him before it; that as the one party in this opposition (Greek philosophy) remained the same after as before the Reformation, so also the other remained the same; and that the un-Greek element in modern has the same source as the un-Greek element in medieval philosophy: namely the Christian revelation. There is hardly a stronger argument for the truth of this contention than to draw the consequences of denying it. If we deny it we must suppose both that the un-Greek (*i.e.*, specifically modern) element in modern philosophy was without a source, and that the un-Greek (*i.e.*, specifically

¹ This is brought out with fine lucidity in É. Gilson's *L'esprit de la philosophie médiévale*. But my whole article is a protest against Gilson's further assumption, that we must look to a resurrection of Scholasticism for a continuation of this great task, and against his implied judgment that the work of the classical modern philosophers represents a declension from the path upon which medieval philosophy set out.

medieval) element in medieval philosophy was without an issue. This supposition can hardly even be entertained by one who has not been hardened in the belief that the history of philosophy begins again *de novo* with Descartes.

To say this is by no means to deny that there was a crisis in the history of thought at the time of the Reformation or that modern differs from medieval philosophy in vitally important respects. The effect of the Reformation in the sphere of thought was analogous in two ways to its effects in the sphere of conduct. In the latter sphere it had the effect, first, of extending the Christian order of conduct from the religious (*i.e.*, monastic) to the secular life. This involved, of course, the disappearance of the 'religious' life as such, but by no means therefore of the principles by which it had been governed. They continued to be applied, with a rigour only intensified by their diffusion, in the Puritan asceticism of the economic life.¹ The Reformation had the consequence, secondly, of transferring the direction of conduct from the external authority of the priest to the internal authority of conscience. But conscience only imposed from within the same laws of conduct which the priest had imposed from without. The Reformation marks a term in the education of the Christian peoples analogous to that which Aristotle proposes as the end of the ethical training of the individual. The first stage in the acquisition of virtue by the individual is his submission to certain principles of conduct prescribed by another, but the end of this submission is his acquirement of a disposition to act in accordance with these principles. When this is achieved, he is emancipated from his tutelage, and his actions are determined henceforth from within himself. But this does not in the least imply that his actions are now liberated from the control of the principles to which they were formerly submitted. It means simply that they are now animated by these principles whereas previously they were conformed to them. Similarly, at the Reformation conduct was emancipated not from direction by Christian principles, but only from their external prescription. Conscience itself was an 'acquired disposition', informed by submission to that very prescription, and if its possessors could mistake it for a 'natural' faculty, that was only because it had been acquired so thoroughly.²

¹ I refer especially to Max Weber's great work, *Die protestantische Ethik und der Geist des Kapitalismus*.

² "Der gesunde Menschenverstand und das natürliche Gefühl roher Türken zum Mass-stab genommen, gibt abscheuliche Grundsätze. Wenn wir aber von gesundem Menschenverstand sprechen, von natürlichem

In the sphere of thought the Reformation had effects analogous to both of these. In the first place (I am reversing the order), philosophers claimed for reason emancipation from the authority of faith, to which it had been so long submitted. They did not realise that the reason for which they claimed autonomy was a reason itself informed by this very submission, and that what they called 'common sense' or 'the natural light' was only an internal revelation of what had previously been revealed externally to faith. If the reason upon which they relied had been in fact what they took it for, a 'natural' faculty bereft of the enlightenment of the Christian revelation, it could have discovered no truths not discovered by reason to the Greeks, and could not therefore have laid down the foundations upon which modern science was raised.

The delusion of the early modern philosophers that their philosophy was based wholly on the evidence of reason¹ (if they were Rationalists) or of experience (if they were Empiricists) prevented them from looking further for the source of their doctrines, or from so much as entertaining the supposition that they were indebted to Christian revelation. But it has been open to no succeeding philosopher to share the delusion. The work of criticism very speedily showed that neither the Rationalist nor the Empiricist philosophy was really based upon the evidence upon which it pretended to rely. No experience, to take one example, could serve as evidence to Locke of the existence of material substances, nor any reasoning demonstrate to Descartes the existence of a material world. No doubt, the assurance of 'common sense' might suffice for the one, and of the 'natural light' for the other. But then it must be admitted that "common sense" is something other than sense and the "natural light" something other than reason; and the way is open for the

Gefühl, so hat man dabei immer im Sinn einen gebildeten Geist; und die, welche die gesunde Menschenvernunft, das natürliche Wissen, die unmittelbaren Gefühle und Offenbarungen in ihnen zur Regel und Mass-stab machen, wissen nicht, dass, wenn Religion, das Sittliche, Rechtliche sich als Inhalt in der Menschenbrust findet, dies der Bildung und Erziehung verdankt wurde, die nur erst solche Grundsätze zu natürlichen Gefühlen gemacht haben." Hegel, *Geschichte der Philosophie*, III., ii., 2; *Werke*, 2nd ed., vol. XV., p. 439.

¹ When Raymond de Sebonde declares in the prologue to his *Theologia Naturalis sive Liber Creaturarum*, that the exercise of the natural reason upon the Book of Nature suffices a man to know without difficulty "whatever is contained in Holy Scripture" (C. C. J. Webb, *Studies in the History of Natural Theology*, p. 292 ff.), that is only an extreme form of the delusion shared in some degree by all the modern Rationalist philosophies.

enquiry: What is the source of that certainty which is derived neither from reason nor from sense? ¹

It will not be enough to show that this certainty had its source in the Christian revelation; it has to be shown also that it had its issue in the establishment of the presuppositions of modern natural science. That these presuppositions are not themselves established by the evidence either of reason or of sense, any acquaintance with the "problem of induction" or with Hume's difficulties about causation is sufficient to show. And in fact the criticism to which the Rationalist and Empiricist philosophies were subjected, in divesting them of all those conclusions to which they were not *upon their own premises* entitled, did divest them of every certainty which the procedure of modern natural science requires for its justification. If these philosophies had never laid themselves open to that criticism, if they had begun by resigning themselves to the scepticism to which they were ultimately reduced; or if, having laid themselves open to it, they had succumbed to it too soon—they would not have performed the function which in fact they performed in the establishment of modern science. What prevented them from succumbing sooner was their reliance upon the revelation which had raised them above scepticism in the first place. Regius and Malebranche, for example, being unable to defend against criticism Descartes' demonstration of the existence of the material world, do not therefore surrender the doctrine; they only recur overtly to the authority of revelation to establish a truth which Descartes had referred to the deliverance of the natural light. The very ease with which this transition is made is sufficient to indicate that Descartes' 'natural light' was informed by the same revelation.

¹ Mr. A. K. Stout ("Descartes' Proof of the Existence of Matter", *MIND*, April, 1932) has argued that Descartes' own doctrine is not that the existence of the material world is assured directly by the 'natural light', but that it is assured directly by something which Descartes distinguishes from the 'natural light' as the 'teaching of nature', and by the 'natural light' only indirectly, inasmuch as it is competent to establish the general veracity of the 'teaching of nature' (though not the truth of any particular one of its dictates).

Acceptance of Mr. Stout's conclusions (which I am by no means disposed to question) would necessitate a certain revision of my terminology, but not any essential modification of my argument. However significant it may be that Descartes should have admitted the existence of a source of certainty other than reason, the admission is practically nullified by the proviso that the general veracity of the 'teaching of nature' must be demonstrable by reason. The proviso makes the certainty of the existence of matter to depend ultimately, if not immediately, upon the 'natural light'.

The time came much later when the appeal to revelation lost the power of directing thought. Kant, who was perhaps the first to perceive quite clearly that the *whole* of the ontological doctrines of modern Rationalism were covertly dependent upon the authority of revelation, regarded this as a sufficient ground for dismissing them, and not as a confirmation of their truth. But by this time the 'dogmatic' philosophies had done their work. A body of natural sciences had arisen upon the presuppositions which they had laid down, and it was possible *now* for the philosopher to establish the presuppositions by the 'critical' method of working back to them from the sciences which were based upon them. During the whole period in which the modern natural sciences were in an early stage of growth the influence of religious authority upon philosophical thought was consistently exerted to preserve it from conclusions, whether sceptical or otherwise, which would have been incompatible with the possibility of these sciences; and religion surrendered this control only when the sciences were established firmly enough to serve in their turn as a datum for philosophical speculation. I will give an illustration at the risk of anticipating what belongs later. Descartes and Kant both reject final causation in nature, but their arguments differ significantly. Kant argues in effect from the absence of final reasoning in science to the absence of final causation in nature; nature must be without final causes because it is presupposed to be so by the science of mathematical physics. But Descartes proceeds in the reverse direction. The avoidance of final explanations by the physicist is not cited as a fact, but prescribed as a rule. The scientist, he says, *ought* to abjure the search for final explanations *because the purposes of God are inscrutable*. This argument is an enthymeme of which the premises to be supplied are that nature is *created* by God, and that the activity of creation is not directed by an intelligible purpose. So that Descartes' prescription to the physicist is based upon the metaphysical implications of Christian dogma.¹

¹ The same connection may be illustrated by another example. Of Aquinas's presentation of the doctrine of the 'star-moving Intelligences' Prof. Webb remarks that "the chief interest to us of these speculations . . . lies in the fact that Thomas Aquinas is so thoroughly alive to the danger involved to the *religious principles of Christianity* in the acknowledgement of the divinity of the heavenly bodies" (*Studies in the History of Natural Theology*, p. 274. My italics). Acknowledgement of their divinity was the basis of the distinction between Celestial and Terrestrial physics, with the abolition of which modern physical science may almost be said to have begun. There could hardly be more striking evidence of the truth of my thesis than the fact that this criticism was first undertaken in the interest of the religious principles of Christianity.

In the second place, as the Reformation in the practical sphere had the effect of extending the application of Christian principles of conduct beyond the religious to the secular life, so in the theoretical sphere it carried out the implications of Christian doctrines beyond the sacred into the profane sciences. The mediæval philosopher had of course believed the Christian doctrine that nature is created. But the belief had been efficacious only in his theology. In his science of nature he had continued to seek for final causes, to define essences and to deduce properties : in a word—he had continued to employ the methods of Aristotelian science, entirely oblivious of the fact that Aristotle's science was based upon the presupposition that nature is not created. The modern investigators of nature were the first to take seriously *in their science* the Christian doctrine that nature is created, and the main differences between the methods of ancient and the methods of modern natural science may be reduced to this : that these are and those are not methods proper to the investigation of a created nature.

With this we may turn to a closer examination of the particular question. We have to determine, first, what the differences are which distinguish the methods of modern from those of Greek natural science ; we have to show that these differences depend upon differences between the modern and the Greek philosophy of nature, and that these in their turn are derived from the differences between the Christian and the Greek conception of God and of God's relation to the world.

I have said what I shall mean by the term 'modern science of nature', but it might appear a difficulty to determine what is to be meant by the contrasted term 'Greek science of nature'. Greek science of nature was in most of its branches an attempt rather than an achievement, and an enquiry into its character might seem to be surrounded by all the difficulties which attend an investigation of the rudimentary and the embryonic. Even to determine what its methods were might seem to require an antiquarian learning which I am far from possessing and which could in any event hardly promise to yield results of philosophical importance.

I shall not embark on such an investigation and my purpose does not require that I should do so. We need not elicit the principles of Greek science from the vestiges of Greek sciences, because we possess a classical formulation of the principles in the Aristotelian Logic. By Greek science I shall mean such science, or attempted science, of nature as conformed to the canons of Aristotelian Logic ; and I shall not be disturbed by the

fact, if it be one, that the Greeks developed some sciences not so conformable; or that the systematic attempt to apply Aristotelian methods to the investigation of nature was characteristic rather of the medieval scholastics than of the Greek philosophers. The peculiar characteristics by which modern is to be distinguished from Greek natural science may consequently be determined simply as those which render the former unconformable to the canons of this logic.¹

Judged by this criterion one of the most important and striking differences, though no doubt it is not the only difference, between the methods of modern and those of ancient natural science is the presence in the former of an empirical element lacking in the latter. Modern science describes natural substances instead of defining them, it discovers their properties by observation and experiment instead of by 'intuitive induction' and demonstration, it classifies their species instead of dividing their genera, it establishes between them the relation of cause and effect instead of the relation of ground and consequent. In each case the modern procedure will be found to differ from its ancient counterpart by the part which sensuous experience plays in it. This is not to say that sensuous experience played no part in ancient science, but that it played a different part: it supplied the illustration but not the evidence of the conclusions of science.²

All the peculiarities of Greek natural science are derived from the assumption that the essence of a natural object is definable, as the essence of a geometrical object is. Once let this be granted, and it follows that the properties must be deducible by reason from the essence, the species derivable by reasoning from the concept of the genus, the necessary connections between it and other objects such as can be perceived by reason to be involved in the essence; it follows, in a word, that empirical evidence must be inadmissible in the same degree and for the same reason in establishing the conclusions of natural science as it obviously is in establishing the conclusions of Euclidean geometry.

The methods of Greek natural science thus depend upon the assumption that the essences of natural objects are definable. What does this scientific assumption presuppose about the nature of the physical world?

Definition is an act of reason containing no element of sense, however necessary it may be that sensuous perception should

¹ Cf. in this connection C. R. Morris, *Idealistic Logic*, chap. iv.

² This is not, of course, the point at issue between Aristotle and Plato. They differ only in estimating differently the importance to be assigned to the sensible as illustration.

precede it. No doubt I must have seen lines, or touched them, before I can define the line. But when I have reached a definition, then 'the line' which I have defined is intelligible only, neither visible nor tangible. That in objects which is intelligible as distinct from sensible is what the Greeks called their form as distinct from their matter. That the form of things is intelligible, and therefore definable, does not of itself constitute the whole of the assumption required to justify the procedure of Greek science, namely that the *essence* of things is intelligible, and therefore definable. It needs the complementary assumption, which the Greeks also made, that the form of things is their essence, *i.e.*, that of the two elements, formal and material, of which every actual thing is composed, the form alone makes the thing to be what it is, whereas the matter contributes no positive element to its being. Matter is the correlative, in the object, of sense in the subject, as form is the correlative of reason; and thus the Greek assumption about science, that there can be no empirical evidence for scientific conclusions, depends upon the Greek assumption about nature which may be loosely designated the assumption of the 'unreality of matter'. The designation is loose, because it is not meant simply that matter is not actual except in union with form; for it is true equally, at least according to Aristotle, to say that form is not actual except in union with matter. What is meant is that the *σύνολον* of matter and form, which alone is actual, is determined to be what it is wholly by the one element of form. The object is *nothing more than* a realisation of form; its matter is the source of no being in it over and above that which it derives from its form, it is the source only of the imperfection with which the latter is realised. The method of Greek natural science thus involves a theory of nature according to which the actual world is distinguishable into the two elements of form and matter, the former intelligible, the latter sensible. Because the 'intelligible nature' is the ground both of all being and of all action in the actual world, whereas matter accounts only for diminution of being and impediment of action, it follows that intelligent comprehension of form is sufficient for the understanding both of what is and of what happens in the actual world, so far as this is capable of being understood, whereas sensuous experience represents no addition to, but only defect of, such understanding.

We have to ask finally what theory of God is presupposed in this theory of nature, and here I shall invert the natural order of investigation by stating my conclusion first. The theory of nature presupposes that neither of the two elements of which

nature is composed is dependent for its being upon a power outside nature, *i.e.*, that neither of them is created. If matter were created it would possess a positive being, if form were created it would not be intelligible. The twin Greek doctrines of the 'unreality' of matter and the intelligibility of form imply that matter and form are alike eternal. We may say in advance, then, that any development of Greek theology, if it is to remain consistent with the presuppositions of Greek natural science, must stop short of the attribution to God of an omnipotent power over nature. Nature may be conceived as dependent upon a supernatural power for the activity by which its two elements are conjoined, but not for the being of either element. I shall endeavour to show, in the briefest possible outline, how Greek theology observes this limitation even in its highest developments, and I shall make some remarks upon each in turn of the three following Greek theological conceptions: (i) the conception of God as identical with nature, or of nature as itself divine, (ii) the conception of God as subject of a purely theoretical activity, (iii) the conception of God as artificer or Demiurge of nature.

(i) The identification of God with Nature finds its earliest expression in the deification of natural powers which is characteristic of the Greek polytheistic religion. So long as this identification is both naïve and complete, so long, *e.g.*, as the god is simply not distinguished at all from the natural object, it does not seem, indeed, that the religion founded upon it can give rise either to a theology or to a science of nature. But Greek¹ religion, though it may have begun with such a naïve identification, did not end with it. The withdrawal of the Gods to

¹ I am using the term 'Greek' with an arbitrary limitation of meaning. By 'Greek religion' I mean the Greek Olympian religion, by 'Greek philosophy' the tradition of philosophy which began with Socrates and culminated in Aristotle, by 'Greek natural science' the science of nature based upon that philosophy, the actual pursuit of which was perhaps rather characteristic of Medieval Scholasticism than of the Greeks themselves. I need hardly say that I do not intend to deny the existence of what I ignore. There was, of course, a Greek religion other than the Olympian, a Greek philosophy before Socrates (there seems to have been a close connection between pre-Socratic philosophy and extra-Olympian religion), and there were at least the rudiments of a Greek natural science which was not a science of formal causes. Reaction against Aristotle in the early-modern philosophers was often enough accompanied by a renaissance of the theories of pre-Socratic philosophers. It remains none the less true that the reaction derived its force from Christian dogma, and only its watchwords from the pre-Socratics. These doctrines were revived and others discarded because these were more readily conformable to the doctrine of Creation.

Olympus implies the recognition of *some* distinction between the natural and the divine. This is no absolute distinction; if it had been, Greek religion would have cast off at a stroke the character which distinguishes it as pagan from either the Jewish or the Christian; but it was sufficient to entail that the sensible object should be regarded henceforth not simply as the god, but as the *appearance* of the god, and its growth or motion rather as the *manifestation* of a divine activity, than as being itself divine.

This partial distinction between God and nature supplied the foundations of Greek science, for the Greek did not free himself from the teachings of his religion when he became a philosopher. The attitude of belief, no doubt, gave way in him to that of understanding, but what he now understood was only what he had previously believed. The great philosophical distinction which Socrates initiated and Plato worked out between the idea and the sensible object was only the explication of the distinction which had been already made in Greek religion between the God and the sensible object.

It will hardly be denied that this philosophical distinction was the foundation of the Greek science of nature, and if it be granted that the possibility of Greek natural science depended ultimately upon the distinction between God and nature achieved even by Greek religion, there may be a readier acceptance of the thesis that the far higher development of modern natural science depends upon the far deeper distinction between God and nature achieved by the Christian religion. The limitations of the pagan distinction are reflected in the peculiarities of Greek scientific procedure. If the gods are to be distinguished from nature, and yet not completely distinguished from it, they must be conceived as *appearing* in nature and as natural objects. The same difficulty concerning the relation of the sensible to the supersensible arises within the Platonic philosophy, and the solution of it is the same: the sensible is related to the idea as appearance to that which appears. The application of these categories to nature implies that the sensible (which is the material) is, *quâ* sensible and material, merely apparent, and this implication justifies the *a priori* methods of Greek natural science. But the doctrine of Creation implies that the material is real *quâ* material.

It is true that the doctrine of nature implicit in Greek polytheism is not of itself sufficient to supply the presuppositions even of Greek natural science. That the forms should be isolable in thought from the accidents of their material embodiment, is not sufficient to constitute them proper objects of a science. A scientific understanding (as distinct from a still quasi-æsthetic

contemplation) demands that its objects be perceived to be inter-related one with another as members of a single system,¹ and this involves a view of the universe different from that involved in any mere polytheism. On the other hand, it does not involve any form of Theism, or belief in a God transcending nature. Nature must be conceived as a unity, but the principle which constitutes it one need not, for any of the considerations yet advanced, be held to possess an existence apart from nature, or to be related to the multiplicity of natural objects in any other wise than that, *e.g.*, in which the principle of life in an organism is related to its bodily members. Though this principle of unity may be termed 'God', it is God only in the sense in which that term is compatible with Pantheism, or a God still imperfectly distinguished from nature. The Greek, in other words, in becoming a monotheist did not necessarily thereby cease to be a pagan; and Pantheism is no less incompatible than polytheism with the attribution of reality to sensible particulars.

(ii) There are Greek theological doctrines which transcend the limitations of Paganism. I shall content myself here with considering two of these, with pointing out in what respects they differ from the doctrine of God as Creator, and with trying to show that it is precisely in virtue of these points of difference that they are enabled to remain compatible with the Greek theory of nature, especially in the two crucial regards which I have mentioned.

The first of these is Aristotle's conception of God as First Mover. It is not without significance for my thesis that Aristotle's proof of the existence of a transcendent God is based upon the necessity of accounting for the communication of motion by efficient causes in nature; in other words, that he approaches most nearly to the Christian doctrine of God at the very point at which his conception of nature approximates most closely to that of modern physics. But Aristotle's God, though admitted to be transcendent, is bereft of any power over nature except the single power of originating motion. Neither the matter nor the form of natural objects depends on him; and even of motion in nature he is not himself the efficient but only the final cause. He is not the source of energy in nature; that must be held to arise within nature from the active potency of the form to realise itself; but is only the end upon which all energy in nature is directed. The only activity of which God is the source is his own

¹ The possibility of syllogistic inference in especial depends upon the systematic interrelation of species.

theoretical activity ; and this activity terminates not upon the world but upon himself.

It may well be questioned whether Aristotle's restriction of God's operation upon the world is really consistent with his argument for God's transcendence ; whether, in other words, that argument does not demand the conclusion that motion in nature has a source as well as an end outside nature. However this may be, it is certainly that restriction which enables Aristotle to retain essentially unmodified the conception of nature already outlined. Nature owes God nothing except that harmony of its operations one with another which they derive from their direction upon a single end ; and which might in fact be as well accounted for by the Pantheistic hypothesis, that nature is animated by a single soul.

The attribution to God of an activity of will sweeps away this restriction, and with it the possibility of maintaining the Pagan conception of nature as self-dependent.

(iii) There is one Greek doctrine of God which ascribes to him a power of efficient causation in the constitution of the actual world. This is Plato's doctrine of the Demiurge or Artificer, and because this, of all Greek theological doctrines, bears the closest superficial resemblance to the Christian doctrine of Creation it will serve best to throw into relief the essential contrast which still persists between the conception of God as Creator and any conception of the divine activity which is consistent with the presuppositions of Greek natural science. The doctrine that God is a Demiurge is perfectly consistent with them, because the activity of a Demiurge (the activity which the Greeks called *Techne*) is essentially both (i) *informative* and (ii) *purposive*, that is to say, it is (i) confined to the information of a given matter, and (ii) directed by the antecedent conception of an end. The activity consists in the realisation in matter of the end, which becomes by realisation the form or essence of the object produced, but since the form must be conceived by the workman *before* he starts his work it cannot derive its being, but only its embodiment, from his activity. The form must be "given" to the Demiurge no less than the matter of his work ; thus, if God is Demiurge of the actual world, his work is confined to the uniting of its two elements, form and matter, but cannot extend to the bringing into being of either element.

The ascription to God of the activity of a Demiurge is thus compatible with the fundamental assumption of Greek natural science, that form and matter are eternal. We may, indeed, see more vividly what is involved in this assumption if we reflect

that to make it is to attribute to natural objects a *constitution identical with that of the products of a Techne*. Plato in the *Timæus* may be unique in asserting that the natural world is the product of a Demiurge; but Aristotle asserts,¹ and all the methods of Aristotelian science presuppose, that natural objects *are as though they were* the work of a Demiurge.

We may illustrate the connection between this presupposition and those methods by an analogy. Any product of one of the useful arts is clearly and indisputably the work of an artificer. If we imagine an investigator (say an archæologist who has uncovered the remains of an unknown civilisation) confronted with a collection of unfamiliar artefacts, it will be possible for him, provided only that he knows them to be artefacts, to institute an enquiry into them by an application of the very methods which Aristotle thought proper to a study of nature.

His first task will be to determine what the different objects are, or to define them; the initial assumption that they are products of an artificer involves the consequence that they are capable of definition. His method of determination will be that of intuitive, not of empirical induction, and what he determines will be the real, not the nominal essence of the objects. He will collect the greatest possible variety of examples of each kind, and will observe their sensible qualities, but his procedure will not be that of the empirical scientist as Locke, *e.g.*, describes it in his doctrine of Abstraction. He will not tabulate the sensible qualities which all his examples have in common, assign a general name to such a complex of qualities, and determine to call by that name in future every object which shall be found to possess all of them. On the contrary, he will use his variety of sensible examples as the geometrician may use a variety of drawn figures, strictly as illustrations, and to facilitate his passage by an act of intuitive reason to a comprehension of something which is not itself sensible at all, but is the reason (*λόγος*) of the object.² What is comprehended will be at once the end which governed

¹ Cf. *Physics*, II., 8, 199a, 12. *εἰ οἰκία τῶν φύσει γιγνομένων ἦν, οὕτως ἂν ἐγένετο ὡς νῦν ἀπὸ τέχνης· εἰ δὲ τὰ φύσει μὴ μόνον φύσει ἀλλὰ καὶ τέχνῃ γίγνεται, ὡσαύτως ἂν γίνετο ἢ πέφυκεν*, and *ib.* 6, 30. Natural objects differ from products of art according to Aristotle only in the one respect, not relevant to the present issue, that they have their principle of action within them.

² "We found cuttings in the rocks which puzzled us for a long time, till I, who had seen the same in Syria, discovered that they were winepresses" (*Letters of Gertrude Bell*, I., p. 240). This discovery was not a detection by any of the senses of a sensible quality which had hitherto eluded them; what is discovered could not have been rendered *visible* by any microscope.

the design of the artificer, and at the same time the form of the product (since it is clearly that in the product which the artificer added to his materials, *i.e.*, is that element in it which is to be distinguished from the material). It will be the real essence, because the end conceived by the artificer will in fact have caused the product to possess the qualities (its peculiar spatial configuration, *e.g.*), which it is found to have ; and hence discovery of the essence will enable the investigator to understand the reason of what he had previously only observed to be a fact.¹

The essence once defined can serve as the ground of demonstration of essential properties ; if an object is to serve a given purpose, it must possess such properties as are evidently indispensable to its fulfilment.

Definition of the essence makes possible its subsumption under a genus and its differentiation into subordinate species by the method of Division : a method differing from that of empirical classification in that it proceeds *a priori* by an insight into the essential nature of a thing, not *a posteriori* by comparison of similar sensible qualities.²

That properties should be demonstrable *a priori* of the essence and that species should be subsumable *a priori* under genera, these are the two conditions necessary for the possibility of a Syllogistic inference which should be free from the fallacy of *Petitio Principii*. The investigator we have imagined could make a fruitful use of the syllogism in constructing a science of his manufactured articles.

In a word : their susceptibility of definition makes it possible to apply to manufactured articles all the other Aristotelian methods. The science of nature would conform similarly to the canons of Aristotelian Logic *if nature were the work of a Demiurge*.

For an object to be definable, two conditions must be satisfied : (i) its form must be intelligible, and (ii) its form must be its real essence. Both conditions are satisfied by the products of a

¹ Of a jug, *e.g.*, the experience of his senses can inform him that it has a flat base and a projecting lip ; but only a discovery of its purpose can enable him to understand why it has.

² Thus the unknown artefacts of our illustration could be classified empirically in any of a variety of ways according to similarity of sensible characteristics (colour, *e.g.*, texture of surface or size) by one who did not know their purpose ; or even if they had had none. But the discovery of the one true system of genera and species, according to which a given object is to be classed, *e.g.*, as a kind of lamp, and not as a species co-ordinate with the sauce-dishes which it resembles in appearance : this presupposes knowledge of the purpose of the objects and is achieved by methods different from those of empirical classification.

Techne, and the possibility of an Aristotelian science of nature depends upon the assumption that both conditions are fulfilled by natural objects.

But the doctrine that nature is created involves the denial that natural objects can satisfy either condition.

(i) That the form of an object is intelligible, means that it is distinguishable in conception from the sensible material of its embodiment. The form of an artefact is thus distinguishable, because the activity of the Demiurge who made it was purposive, that is to say, was directed by conception of an end. What he conceived as end, we distinguish as form; and we are enabled to conceive in distinction from sensible accidents precisely so much as he conceived in advance of his execution.

But the work of creation is not purposive; and as there is no end distinctly conceived by the creator in advance of his execution, so there is no form distinguishable by us from the accidents of its embodiment. This may be most easily seen in the contrast of fine or creative art with the activity of a Demiurge or artificer. It is notorious that the creative artist, *e.g.*, the painter, has no clear knowledge of what he is going to achieve before he has achieved it; and the critic on his side, when confronted with a work of creative art, is indeed aware that there is 'something more' in it than the sensible material—a great painting is more than a certain complexity of coloured surfaces—but this 'something more' (we may call it loosely 'the meaning') is *not* capable of being conceived in distinction from the sensible material in which it is expressed. The meaning of a painting is not intelligible in the sense in which the purpose of a wheelbarrow is.

The form of natural objects would be distinguishable (and the objects therefore definable) only if the activity of God were purposive, *i.e.*, directed upon an end which is not itself the product of his activity. But if God is a Creator, natural objects can have no form distinguishable as the object of the intellect.

(ii) The doctrine of Creation attributes to God an autonomous activity of will. No doubt it is also implied in the conception of God as a Demiurge that he is the subject of some practical action. His work is not exhausted according to this doctrine in the theoretical contemplation of the forms, but he engages beyond that contemplation in the non-theoretical activity of embodying them. But it is characteristic of the work of a Demiurge that in it the practical is wholly subordinated to the theoretical activity. The entire activity of the craftsman, in so far as he is a craftsman, is dictated by the end or plan which is the object of his theoretical conception. No doubt the will of any human artificer may escape

from this dictation by his reason ; he may add details to his work which are not necessitated by the dictates of his craft (if he is a bad workman) or are even contrary to them (if he is a corrupt one). But this insubordination of will is a mere defect and simple failure to achieve the perfection of an artificer.

That in an artificial object which is not necessitated by its idea is the *contingent*, and just as the insubordination of will is nothing but an imperfection in the artificer, so the presence of the contingent is nothing but a defect in the artefact.

Bad workmanship is not the only cause of contingency in the product. This may arise also from recalcitrance of the material ; and since bad workmanship cannot be argued in the divine Demiurge, contingency in the natural world must be attributed to this source. Natural objects are contingent, *i.e.*, they fail to conform to their idea, precisely in so far as they are material.

Now if natural objects either are artefacts (according to the theory of the divine Demiurge) or are (according to the Aristotelian theory) in this respect analogous to artefacts that they are nothing but an embodiment of form, then the unavoidable element of contingency which they derive from their matter is nothing but a defect of their being. It does not make them something more than an embodiment of form, but makes them only a bad embodiment of form ; just as two inches more on one leg of a table does not make it more than an artefact, but only a bad artefact.

Objects are intelligible in so far as they are informed, sensible in so far as they are material. The contingent, therefore, or that in them which is not derived from their form, is sensible only, without being intelligible. But since the contingent has been found to represent only a defect of being, it will follow that natural objects are sensible only in so far as they fail to achieve their being. That in them which constitutes them objects of sensation is no increment, but only a defect of their intelligible nature ; and therefore sensation can contribute no evidence concerning the nature of the thing which should be additional to what is perceived by reason. As the being material is a defect and not an increment of being, sensation is an imperfection of knowledge,¹ not a way of knowing.

The absence of an empirical element in Greek natural science follows from this.

But the will of the maker can be subordinated to his reason, as the will of the Demiurge is, only so long as "making" is identi-

¹ It is at the very most the *occasion* of knowledge.

fied with formation, because form alone can be the object of reason. In the creative act the will must exceed any regulations which reason can prescribe. That is to say, the 'insubordination' of will to reason, which could be only a defect in God so long as God is conceived as Demiurge, becomes essential to his activity so soon as he is thought of as Creator. It is what constitutes him, not a bad Demiurge, but something altogether more than a Demiurge.

The *voluntary* activity of the Creator (*i.e.*, that in his activity which exceeds determination by reason) terminates on the *contingent* being of the creature (*i.e.*, on that element of its being which eludes determination by form, namely its matter and the characteristics which it possesses *quâ* material). If such voluntary activity is essential to God, it follows that the element of contingency is essential to what he creates. So soon as nature is conceived to be created by God, the contingent becomes more than an imperfection in the embodiment of form; it is precisely what constitutes a natural object more than an embodiment, namely a creature.¹

But the contingent is knowable only by sensuous experience. If, therefore, the contingent is essential to nature, experience must be indispensable to the science of nature; and *not* indispensable merely as a stage through which the human scientist must pass on his way to attaining adequate knowledge by reason, but indispensable because knowledge by reason cannot be adequate to a nature which is essentially something more than an embodiment of form. This 'something more', the element in nature which depends upon the *voluntary* activity of God, is incapable of becoming an object to reason, and science therefore must de-

¹ I suggest that we use the term "real" to attribute to a thing the being which is proper to a created object. Its meaning differs from that of the Greek *ὄν* precisely as created from uncreated being. That is why, for instance, reality is incapable of degrees, whereas *οὐσία* was capable of an indefinite number of them. What is created *ex nihilo* must be entirely present so soon as it has ceased to be wholly absent; but an object of which it is the whole being to be an embodiment of form, achieves a greater or less degree of being according to the degree of perfection with which form is realised in it. It is not without significance that the term 'real' in its modern sense passed into secular language only after the Reformation (see *O.E.D.*), *i.e.*, at the period at which the concepts of Christianity began to revolutionise the sciences of nature.

Again, the terms 'nature' and 'natural' bear a different meaning from the Greek terms *φύσις* and *φύσει*. The difference is simply that we mean by nature 'created nature', and call 'natural' what is proper to a created nature. We are generally conscious of the difference, but oblivious of its source.

pend, in regard to this element, upon the *evidence* of sensation. The reliance upon the senses for evidence, not merely for illustration, is what constitutes the empirical character peculiar to modern natural science; and the conclusion follows that only a created nature is proper object of an empirical science.

What we have attempted to show is that the method of natural science depends upon the presuppositions which are held about nature, and the presuppositions about nature in turn upon the doctrine of God. Modern natural science could begin only when the modern presuppositions about nature displaced the Greek (this was, of course, a gradual process, but its crisis occurred at the date of the Reformation); but this displacement itself was possible only when the Christian¹ conception of God had displaced the Pagan, as the object (not merely of unreasoning belief, but) of systematic understanding. To achieve this primary displacement was the work of Medieval Theology, which thus laid the foundations both of much else in the modern world which is specifically modern, and of modern natural science.

Creative activity in God, material substance in nature, empirical methods in natural science—how closely each of these three involves the other is made clear by an examination of almost any of the great philosophies of the early modern period. A defect in the philosophical conception of God is reflected in corresponding defects both in the doctrine of nature and in the theory of natural science. Thus it is a mark of the philosophy of the Rationalist tradition that it is unable wholly² to digest that un-Greek ele-

¹ I mean Christian, not Jewish. The Christian doctrine of God derived much from the Greek and thus included within itself, besides much from Jewish sources, much also from the very doctrine which it displaced. Cf. p. 468 *inf.*

² The qualification is to be emphasised. Modern Rationalism differs markedly from Greek Rationalism in its theories of God, nature and science, and the differences are due, as I have illustrated above, by the example of Final Causation, to its absorption of the truth of Christian doctrine. What I am maintaining here is that this absorption was still incomplete.

I must stress the fact that the limitation of the scope of this essay precludes me from doing justice to the philosophy of modern Rationalism. I have confined myself in the main to a single characteristic (the presence of an empirical element) by which modern differs from ancient natural science, and I have endeavoured to show its connection with a single Christian doctrine (that of the Creation). It is the essence of the *Empiricist* philosophy of nature to stress that element of natural objects which exceeds the grasp of the intellect, and it is easy therefore to give the impression that Empiricism alone is adequate either to exhaust the truth of Christian doctrine, or to supply the pre-suppositions of modern natural science; while modern Rationalism succeeds in doing either, if at all, only in so far

ment in the Christian theology according to which God is endowed with a *voluntary* activity in the creation of the world. Descartes' 'clear and distinct idea' of God is the idea of an infinite *thinking* substance, and although the influence of Christian dogma is strong enough in many places to modify his language, so that, having proved the existence of God, he proceeds to attribute to him activities other than theoretical, what constitutes him Rationalist is precisely that this attribution is not more than verbal. Christian dogma works in him strongly enough to modify his language, but not strongly enough to transform his thought. The God of which he has *demonstrated* the existence is a God whose whole essence is to think. His Rationalist doctrine of nature corresponds with his Rationalist doctrine of God: as he cannot conceive a voluntary activity in God, so he cannot conceive the reality of a contingent element in nature,¹ and his identification of matter with extension is the inevitable consequence of his identification of the divine activity with thought. Spinoza carried the Rationalism of Descartes to its logical conclusion. He explicitly denied those elements both in the activity of God and in the being of nature, which Descartes had failed to conceive clearly, but which² the influence of Christian dogma had been powerful enough to prevent him from denying. It is obvious that the Rationalist doctrine of nature is incompatible in its turn with the presuppositions of empirical science. If the contingent in nature is condemned to the status of appearance, sensation can make no positive contribution to knowledge; and the only natural science possible upon the presuppositions of Spinoza's philosophy would be a science which should be, like Spinoza's 'Ethics', *more geometrico demonstrata*.

The Rationalist philosophy of nature had to be corrected if it was to be rendered consistent with the possibility of an empirical natural science. From what source could the correction come? The time had not yet arrived when it was possible to argue back from the existence of a body of natural science to the nature which it presupposed as its object. Neither could direct inspection

as it has absorbed some of the truth of Empiricism. But modern differs from ancient natural science in other respects besides the part played in it by experience, and Christianity has other doctrines relevant to a philosophy of nature, besides that of Creation. My argument does not exclude either the possibility that modern Rationalism does justice to some features of modern natural science which Empiricism ignores or even that it has absorbed the truth of some Christian doctrines which Empiricism has neglected.

¹ Cf. e.g., *Princ.*, II., viii.: "That quantity and number differ only in thought (*ratione*) from that which has quantity and is numbered".

² At least the former of which.

of the natural world afford evidence either to support or to disprove any theory of its metaphysical constitution. There was no standard by which the Rationalist doctrine of nature could be corrected, there was a standard only for the correction of the Rationalist doctrine of God. That had to be remoulded so as to conform to the Christian doctrine that God is Creator, and this remoulding carried with it as an implicit consequence such a modification of the theory of nature as would have rendered it consistent with the presuppositions of empirical science.¹

It may serve to obviate a misunderstanding, to which I have perhaps laid myself open, if I conclude with a remark on the philosophies of modern Empiricism. Berkeley, to take the example most apt to my purpose, stresses the share of sense in knowledge even to the denial of any share to reason, and he stresses the practical activity of God to such an extent that he would be forced, if he were consistent with himself, to deny to God any but a practical activity.¹ Must it not then be admitted, I imagine

¹ The essential connection which subsists between the doctrine that God has will on the one hand, and that a science of nature must be empirical on the other, may be illustrated clearly by a reference to the Leibnizian distinction between the possible and the actual. Possible is whatever is object of God's understanding and of our 'clear and distinct' (i.e., intellectual, non-empirical) perception. The addition of existence to the possible Leibniz attributes to an activity distinguished from God's understanding as God's will. Existence is not intelligible; and since it is involved in the doctrine of God's will that existence is an addition to, not a diminution of, the being which belongs to the possible, the consequence cannot be avoided that intelligence is *inadequate* by itself to the knowledge of existent nature, and requires to be supplemented by sensation: i.e., that an empirical element is necessary to natural science.

Conversely the rationalist doctrine that sense is only defect of understanding may be seen to be incompatible with the attribution of will to God. According to this doctrine the sensible *is* the intelligible imperfectly known; i.e., it derives its sensible character from the imperfection of human perception, and therefore not from an activity of God.

Leibniz maintains a rationalist epistemology side by side with a voluntarist theology, in spite of their mutual incompatibility. Nothing short of the authority of Christianity could have prevailed upon him to admit the latter doctrine into his philosophy in the teeth of the opposition of the former. If he had but attached yet more weight to this authority, it would have led him to reform his rationalist presuppositions into consistency with his theology, and *thereby* into consistency with the procedure of empirical science.

¹ This implication of Berkeley's philosophy is clearly brought out in Mr. J. D. Mabbott's admirable article "The Place of God in Berkeley's Philosophy", in the *Journal of Philosophical Studies*, January, 1931.

It is significant that Berkeley, like Descartes, is preserved from a consistency of error principally by the necessity of conforming to Christian

the objection, that Berkeley's philosophy has *wholly* assimilated the truth of the doctrine of Creation? And yet Berkeley's philosophy is incompatible with the belief in a material substance, and signally fails to justify the presuppositions of the modern science of nature. How is this to be reconciled with the thesis of an intimate connection between the doctrine of creation and the presuppositions of empirical science?

This supposed objection rests upon the mistaken identification of the Christian doctrine of Creation with the un-Greek element in the Christian doctrine. The failure of modern Rationalism was its failure to do justice to this un-Greek element, the failure of modern Empiricism was its failure to do justice to anything else. The Christian doctrine on this, as on all other subjects, itself includes an element derived from Greek philosophy, and any doctrine from which all Greek elements are excluded is less than Christian. It is Christian to ascribe to God an activity of will, but it is not Christian to deny to God a theoretical activity or to ascribe to him a *blind* activity of will. It is a consequence of the Christian doctrine of Creation that the created world must contain an element of contingency, not that it must be nothing but contingent. It was because he drew this latter consequence, and was unable to attribute to matter the possession of any intelligible (as opposed to sensible) qualities that Berkeley was led to his denial of material substance, and to the conclusion, implicit in his philosophy if not admitted by himself, that a science of nature is not possible. Thus Berkeley falls short equally with Spinoza of expressing in his philosophy the whole of what is contained in the Christian doctrine of God. Spinoza had denied voluntary activity to God, Berkeley denies everything but voluntary activity. Similarly in their doctrines of nature, whereas Spinoza had denied contingency, Berkeley denies everything else; Spinoza's world is a nature, but is not created, Berkeley's is created but is not a nature, and so both are compelled, though for opposite reasons, to deny material substances, which can exist only in a created nature. This denial necessitates finally that both fail equally, again in opposite respects, of consistency with the presuppositions of modern natural science. Of Spinoza's world no science could be empirical, of Berkeley's no experience scientific.

doctrine in his theory of God. He does not shrink from the consequence that the science of mathematical physics is impossible, nor from outraging Common Sense by his denial of material substance, but he cannot allow himself to rest in the conclusion that the divine activity is one of blind will.

IV.—DISCUSSIONS.

THE UNITY OF THE UNIVERSE AGAIN.

THE *dialectical* confutation of Mr. Feuer's attempt to prove the unity of the universe would, I suppose, take the form of arguing that on his own showing there are *two* (or perhaps three) 'universes,' viz. (1) the physical, and (2) the metaphysical. To these he might probably be compelled to add (3) the psychological, and (4) the logical uses of 'universe,' as both are common and important.

I am, however, loth to have recourse to dialectics in dealing with one who has seriously tried to answer a question, which has long stood in need of answering, and with a critic who actually quotes and paraphrases my arguments correctly. I prefer therefore to raise more searching questions, and to inquire whether Mr. Feuer's arguments do not frequently beg the question and sometimes contradict themselves.

The first of these faults would seem to be exemplified by the assurance that "the metaphysical universe is, by definition, the class of *all* existents, and this class, when divided, is no longer exhaustive" (p. 347). This surely assumes that the unity of the universe can be proved by making an astute definition which *claims* to cover all 'existents'; after which no further inquiry need be made as to whether this *a priori* claim is actually borne out by the facts. Clearly this is the notorious procedure of the 'ontological proof' of the existence of God, which has so long shocked not only common sense but also 'honest' metaphysicians like Aristotle and Kant. I will not discuss it further here, but merely ask Mr. Feuer whether he fully realises what he has done.

A second begging of the question occurs when Mr. Feuer identifies my 'totality of reality' with his own "all-inclusive metaphysical universe". For this overlooks the vital difference that for me the totality of reality is a very problematic notion, merely a logical conception to be tried out on the real, whereas his own assumption is boldly and badly *a priori*. That is, he takes for granted that all the alternative constructions (or uses) of 'universe' apply to "the same set of existents". But does not this *assume* that the notion of 'universe' has an 'extensional equivalent,' and moreover that all its exemplifications must apply to *the same* 'extensional entity'? And is not this precisely one of the chief questions in dispute?

Moreover, even if Mr. Feuer were allowed to beg this question, it would not help his case. Even if reality were such that all our various ways of conceiving it did in point of fact refer to one and the same 'extensional entity,' what would this prove? Not, surely, the unity of the conceptual (intensional) 'universe'? It might be merely a (? fortunate) accident that there existed an extensional entity so many-sided or so amiably ambiguous as to let itself be conceived as any one of a number of alternative universes. Intensionally we should be left with a *plurality* of 'universes,' and actual reality would be indeterminate and would *ex hypothesi* refuse to decide between these alternatives. So the notion of universe, being indeterminate, would be useless to argue from. To unify the universe intensionally, we should have to conceive a sort of superior super-universe, of which all our alternative 'universes' could be taken as examples.

A third, and very concise, case of begging the question occurs in the remark "every real known to us is a member of the class of existents". This provides no asylum for alleged reals supposed to be known but turning out to be unreal. And it assumes that 'the class of existents' is exhaustively known.

Lastly, on p. 348 there occurs another attempt to argue from an arbitrary definition, in this case of time, which is then promptly alleged to hold of the real. This completely disregards the real difficulty. No doubt the mathematician may frame an arbitrary definition, and call it that of 'time'. But how can he guarantee that the course of events will conform to his definition? Mr. Feuer has ignored also the many ambiguities of 'time' and 'infinity'.

I could wish also that he had told us what he means by 'meta-physical' and 'whole'. He apparently does not conceive 'wholes' as constituted by the synthesis of their parts, as Kant recommended, and to me therefore his use of the term strongly savours of self-contradiction. He has probably been impressed by mathematical fictions which postulate infinite classes, if not by popular hyperboles which revel in 'infinite' wholes. But all such conceptions only raise once more the problem of the *application* of our ideas to the real. How do we know *a priori* that nature must conform to any arbitrary definition we choose to lay down?

I think it also a little unreasonable that he should take exception to my argument that the spatial aspect of the world cannot be a whole because it has neither shape nor size. For surely *spatial* wholes may fairly be expected to have both, and to assert that "to some wholes the category of shape is inapplicable" appears to be merely irrelevant. I would say the same about the remark that "the existence of a Democritean infinite space" is not confuted. Why should a critic of the traditional confusions between geometry and physics be required to 'confute' it? Is it not enough to point out that the question of the nature of *physical* space is a physical problem

to be investigated empirically, while the question of Euclidean or non-Euclidean space is a *geometrical* one, and the question of what geometry is to be used in physical calculations is a matter of practical *convenience*? The question of whether physical space *is* Euclidean or non-Euclidean once more involves the procedure in dispute, that of alleging that humanly convenient concepts hold of the real; but it is strictly meaningless, as Poincaré explained some twenty or thirty years ago.

Let me conclude however upon a note of agreement with Mr. Feuer's last sentence. I agree that philosophers are not to be blamed for the complexities of the world, though I deplore that so many have introduced unpardonable complexities into their own systems. Nevertheless it is also a grave mistake to attempt hasty and unwarranted simplifications which are not avowed and do not work. The doctrine of the self-evident and indisputable unity of the universe seems to be a palmary example of such a mistaken simplification.

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ONTOLOGICAL REMARKS ON THE PROPOSITIONAL CALCULUS.

WHEREAS there is fairly general agreement relatively to such principles and methods as are involved in the technical development of the so-called theory of deduction or calculus of propositions, on the other hand there is no such uniformity of attitude regarding the nature of the entities to which that calculus is supposedly applied.

Wittgenstein¹ construes the proposition as a sign, namely the sentence; but it is the proposition as the denotation of the sentence, *i.e.* as the entity, if any, whereof the sentence is a symbol, that is the present concern. It is these elusive entities, presumably, that are the elements of the propositional calculus and are denoted therein by the variables "*p*," "*q*," etc., and their combinations. But what manner of things are these, whose names are sentences? Not facts, for that would leave no place for false propositions. Are they then judgments? Or abstract possibilities, Platonic ideas? Or are they merely, as with Frege,² the two truth-values, truth and falsity?

Closely related to the question "What is a proposition?" is the question "When are propositions identical?" or better "When do two sentences denote the same proposition?" The notion of propositions as truth-values occupies an extreme in this respect; under this doctrine the material equivalence of propositions, *i.e.* their agreement in point of truth or falsity, is tantamount to their identity. The opposite extreme would be to demand that no two sentences express the same proposition, *i.e.* that the expressibility of propositions by physically unlike sentences is tantamount to their diversity. An unlimited variety of intermediate views are possible, *e.g.* that propositions are identical if and only if they are mutually derivable by the principles of logic. Whereas in the logistical development of the theory of deduction logicians examine the various truth-value connections of propositions, *e.g.* material equivalence, conjunction, etc., and whereas they examine also the various heuristic relationships of propositions, *e.g.* consistency, relative deducibility, etc., on the other hand such considerations of propositions as would relate to propositional identity or diversity are customarily omitted from the formal developments. Yet logicians frequently entertain the notions of propositional identity and di-

¹ *Tractatus Logico-Philosophicus*, 3-12, 3-31.

² *Grundgesetze der Arithmetik*, Band I, S. 50.

versity in an informal way, *e.g.* when they claim that "*p*" and "*p* is true" (or "not *p*" and "*p* is false") do not represent the same proposition.

Outside discussions of logic we never bestow consideration upon propositions, in the sense of non-sentences whereof sentences are symbols, but engage only in the manipulation of the sentences themselves. We do not, *e.g.*, have occasion to observe that "Boston is east of Chicago" and "Chicago is west of Boston" are (or are not) two names for the same proposition; indeed, whereas we may have occasion to reflect that "Boston" is the name of a city, we do not have occasion to regard "Boston is east of Chicago" as a name of anything whatever. Thus it is that in the theory of deduction, as a formal systematisation of certain aspects of the ordinary use of language and exercise of reason, there is no call to consider what manner of entity a proposition may be or to formulate the conditions under which propositions are identical. Propositions are hypostatised entities, inferred denotations of given signs.

Once we postulate entities whereof sentences are symbols, the logical principles for manipulating sentences become principles concerning the entities—propositions—which the sentences denote. Insofar the theory of deduction becomes a calculus of propositions; but it remains a very partial calculus in that respect, since its only principles are those which governed the manipulation of sentences antecedently to the notion that sentences were names of anything. Hence, while we are apprised of a wide array of logical properties of propositions, concerning which there is little essential disagreement, on the other hand as to the residual character of propositions we have that full latitude of choice which attends the development of gratuitous fictions.

When the theory of deduction is woven into a broad and unified logistical system treating of other topics as well, the structure and primitive machinery of the total system might happen to be improved in point of simplicity and economy by thus construing sentences as denotative of certain entities, "propositions," and then identifying these entities with some manner of definite technical entities which figure also in other aspects of the total logistical system.¹ But such considerations will depend upon the structure of the broader system in question. When, as is ordinarily the case, the theory of deduction is developed and considered in isolation from other parts of logic, the whole notion of sentences as names is superfluous and figures only as a source of illusory problems.

Without altering the theory of deduction internally, we can so reconstrue it as to sweep away such fictive considerations; we have merely to interpret the theory as a formal grammar for the manipulation of sentences, and to abandon the view that sentences are

¹ Such has been my procedure in *A System of Logistic* (Harvard University Press, 1934), Chapter III.

names. Words occurring in a sentence may be regarded severally as denoting things, but the sentence as a whole is to be taken as a verbal combination which, though presumably conveying some manner of intelligence (I write with deliberate vagueness at this point), yet does not have that particular kind of meaning which consists in denoting or being a name of something.

In the theory of deduction the signs " p ," " q ," etc., are customarily construed as proposition variables, *i.e.* as signs ambiguously denotative of propositions, *i.e.* as signs ambiguously denotative of the things which sentences denote. We now cancel this circuit through denoted entities, and explain the signs " p ," " q ," etc., directly as ambiguously abbreviated sentences—which comes to the same thing as before except that the existence of denoted entities, propositions, is no longer presupposed.

The expression " $\sim p$ " is construed in the propositional calculus as denoting the contradictory of the proposition p ; here again we may short-cut the passage through propositions, by construing the sign " \sim " as shorthand for the word "Not" or for the words "It is false that". Thus, where " p " is an abbreviation for a sentence " --- " the sign " $\sim p$ " becomes an abbreviation for the sentence "It is false that --- ". Similarly " $p \vee q$ " ordinarily explained as denoting the alternation of the propositions p and q , comes now to be explained by construing " \vee " as a new spelling of the word "or"; " \vee " is a connective enabling us to build new sentences from old, without question of denotation. In the same way the dot in " $p \cdot q$ " comes to be explained as shorthand for "and," and the sign " \supset " is so explained that " $\dots \supset \text{---}$ " is "If \dots then --- ."

Thus reconstrued the theory of deduction remains unchanged in structure, but ceases to be a system in the usual sense. The usual sort of system treats of some manner of elements, say cardinal numbers or geometrical points, which are denoted ambiguously by variables; operative upon these elements are certain operations or relations, appropriately expressed within the language of the system. The theory of deduction, when construed as a calculus of propositions, is a system of this kind; its elements are propositions denoted by the variables " p ," " q ," etc., and its operations are the propositional operations of denial, alternation, material implication etc., denoted by prefixure or interfixure of the signs " \sim ," " \vee ," " \supset " etc. When, on the other hand, the theory of deduction is reconstrued in the foregoing manner as a mere organon of sentences, it ceases to be concerned with elements subject to operations; the former proposition-variables " p ," " q ," etc., become ambiguous sentences, symbols of nothing, and the signs " \sim ," " \vee ," " \supset ," etc., become connectives of sentences, innocent of operational correlates in the realm of denotations.

Consider, *e.g.*, the theorem " $p \supset (p \vee q)$ " of the theory of deduction. According to the usual interpretation this expression represents an

element of the system, *i.e.* a proposition, built up in the indicated manner from any propositions p and q by the operations of alternation and material implication; the occurrence of the expression as a theorem is as much as to say that any proposition thus constructed is true. When, on the other hand, we reconstrue the theory of deduction so as to eliminate the notion of denotations of sentences, " $p \supset (p \vee q)$ " ceases to denote an element of the system: for the whole notion of the systematisation of elements has dropped out. Instead the expression becomes merely an abbreviation of any sentence of the form "If so-and-so then so-and-so or such-and-such". In enunciating " $p \supset (p \vee q)$ " and the other theorems of the theory of deduction we are engaged merely in affirming sentences which, though ambiguous, are ambiguous in respects immaterial to truth.

From this point of view all speculation as to the nature of propositions drops out. The theory of deduction becomes a paradigm depicting the use of the connectives "or," "if-then," etc., with a view to the truthfulness of the sentences which they generate. There are no inferred entities, no flights of abstraction beyond the realm of everyday uses of words.

It was suggested above that in the ordinary calculus of propositions the theorems are expressions denoting certain of the elements of the system. This is an anomaly upon which mathematicians have looked askance. It is customary to consider systems in abstraction from the nature of their elements; the theorems of a system, thus viewed, become sentences telling us various properties of unidentified elements. But to abstract from the fact that the elements of the propositional calculus are propositions is to deprive the theorems themselves of their character as sentences, since in that calculus the theorems are symbols of elements of the system. The student of systems in the abstract thus comes to an *impasse* when he takes up the calculus of propositions. Hence the mathematician may not be displeased to see the calculus of propositions eliminated, and to find in its stead a theory of deduction proceeding merely on the basis of abbreviations of sentences and making no pretence to being a system in the ordinary sense. Besides the previously noted advantage of eliminating useless lumber and diverting all speculation regarding elusive entities called propositions, the procedure has this further advantage of extruding an anomalous case from the theory of systems.

But there is a way of gaining these advantages without persisting in the exclusion of the theory of deduction from the orthodox realm of systems. The theory can be reinterpreted in such a way that the signs " p ," " q ," etc., resume their status of variables denoting elements of the system, without return to the fiction of propositions as denotations of sentences. We can reconstrue the theory of deduction as a branch of semantic, a system whose *elements* are shapes, signs, specifically sentences. The signs " p ," " q ," etc., thus become

sentence variables; neither signs ambiguously denotative of propositions nor signs ambiguously abbreviative of sentences, but signs ambiguously denotative of sentences. The sign " \sim " of denial comes to indicate a semantic operation, the operation of introducing "not," "does not," etc., properly into the interior of a sentence p . The sign " \vee " comes to indicate the semantic operation of stringing out two sentences and interposing the word "or". Similarly for the rest. The signs " \sim ," " \vee ," " \supset ," etc., are thus no longer condensed spellings for certain adverbs and conjunctions, but are signs of the operations of properly inserting such adverbs and conjunctions.

The expression " $p \supset (p\vee q)$ " is no longer an ambiguous sentence, but a symbol ambiguously *denoting* sentences; it denotes any sentence of the form "If so-and-so then so-and-so or such-and-such". So long therefore as the theorems of a system are to be sentences, rather than names of sentences, " $p \supset (p\vee q)$ " is inadmissible as a theorem; it requires a prefix, say " \vdash ," which may be read as a predicate to the effect that the element denoted in its wake is a true (i.e. truthful, truth-telling) sentence. Thus the so-called assertion sign " \vdash ," customarily used in the theory of deduction as a convenient but formally extraneous tag marking off theorems and postulates, comes now to assume an essential rôle within the system as denotative of a property of sentences. Yet " \vdash " remains confined in the customary fashion to initial occurrence in theorems and postulates: for " \sim ," " \vee ," " \supset ," etc., attach only to names of sentences, whereas " $\vdash p$ " is itself a sentence (about a sentence) rather than a name of a sentence.

If now with Wittgenstein we take "proposition" as meaning simply "sentence," the calculus of sentences just now discussed becomes a calculus of propositions; common usage is thereby superficially restored, to the extent that the theory of deduction is once again a calculus of propositions. But such verbal manœuvres do not affect the real distinction: that, namely, between a system whose elements are sentences and a system whose elements are denoted by sentences. In a calculus of propositions in the former of these senses the propositional operations represented by " \sim ," " \vee ," " \supset ," etc., become semantic operations upon sentences; the notion of entities denoted by sentences goes by the board, and the question of propositional identity comes to admit of one or another definite answer in terms of geometrical similarity or conventional correspondence of written marks. This calculus of sentences is, moreover, a system of the orthodox pattern: its theorems are not expressions of its elements, but statements about its elements, viz. statements that elements (sentences) of such and such form have the property \vdash .

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ARE THERE VAGUE SENSE-DATA ?

IN his book entitled *Perception* Mr. Price writes that a sense-datum "is of exactly just this shade of colour and has just this shape and no other" (p. 150). "When a cricket ball, for instance, is seen from twenty yards off, the sense-datum is flat . . . From a short distance, say two yards, which is within the range of stereoscopic vision, the sense-datum is no longer flat but bulgy . . ." (p. 32). I suspect that Mr. Price is here abusing the word "flat," and that perhaps there is a sense in which sense-data are indefinite or vague. Since Mr. Price denies this, believing that the notions of vagueness and clarity apply to the perceptual act but not to sense-data (p. 149), I am going to venture upon a number of considerations to the effect that some sense-data are vague at least in certain respects, and in the belief that my critical comments will turn out to have a rather important bearing on theory of knowledge in general.

A simple experiment will test Mr. Price's view. In the main, this view we are about to test is that the field of stereoscopic vision contains data which, if well within it, are "given" as facing in various ways—as for example the surfaces of a cubical datum, which do not all flatly face forward in the direction of the percipient. But this field, asserts Mr. Price, has a "maximum depth" (p. 218) in the plane of which all data do "face forward" in the direction of the observer, presenting "flat" surfaces which lie in the plane of maximum depth. Even a die of six sides faces, as sensed, only in one direction—namely, towards the observer—when placed ten or twenty yards off at the maximum depth of stereoscopy.

Now our experiment requires only a cricket ball and a flat cardboard disc of the colour and radius of the ball. (1) Place these side by side, at a distance of two yards from the point of view.¹ In this situation, the frontal shapes of the two data are clearly different, the one bulgy and the other flat, regardless of whether or not they face forward in the direction of the point of view—excluding for the moment the case in which the disc is seen edge-wise. By "frontal shape" I mean the whole of the datum's sensible topography, configuration or relief *lying within* its "profile" or sensible outline. So we analyse the datum into two shapes, namely, its profile or outline and the topography of its expanse within this outline, the latter being the "frontal shape". In this first stage of our experiment, we observe that the frontal shapes are sensibly dissimilar.

(2) Now place the cricket ball at a remove of twenty or thirty

¹Mr. Price defines "point of view" on p. 253, but this is here irrelevant.

yards from the point of view, without increasing the distance of the cardboard disc, but arranging both so as to be simultaneously visible. Examine carefully the frontal shapes of the two sense-data. Mr. Price reports that the frontal shape of the distant ball is now precisely like that of the proximate disc, that is, flat. But after the closest scrutiny of which I am capable I am forced to report that, though the distant ball still has its circular profile and red colour, its frontal shape is now indeterminate. I *sense* it as determinately *neither flat nor bulgy*, and I underscore "sense" because I am perfectly sure that the indeterminacy of that frontal shape is not due to "attribution" (p. 149) or to pragmatic demands for specification on my part. *Qua sense-datum*, the frontal shape of the ball in this second situation is a *determinable*, not a determinate. Before passing on to the third stage of the experiment, I beg Mr. Price to look again to see if the frontal shape of the remote datum is sensibly flat *in exactly the same sense* in which the near disc is flat. The fact that any unsophisticated but cautious and clear-sighted cricket player would say, "I cannot tell from this distance whether that red circular patch on the green is bulgy or flat," must give Mr. Price pause, and the cricket player must not be bullied into asserting that he does sense a flat frontal surface.

(3) Remove the disc gradually to a place beside the distant ball, keeping it in a position that preserves its circular profile for the observer. During this gradual removal, observe the disc carefully, and report on changes in its frontal shape, as well as on subsequent similarities or dissimilarities of ball and disc. Mr. Price reports no changes in the disc's frontal shape, since it was flat within the stereoscopic region and remains so at its maximum depth. But, despite my love of agreement among philosophers, I find I must again disagree with Mr. Price. Whereas I originally could sense the frontal shape (flat) of the disc when it was two yards off, I discover that it becomes more and more indefinite as the disc approaches the distant ball, until finally I simply cannot sense any determinate frontal shape at all. As to resemblances between ball and disc at a distance, Mr. Price reports that both frontal shapes are now alike, because both are flat. I concur with him on the matter of resemblance, but I find it to consist in the fact that both frontal shapes are in this third situation indeterminate, not flat. And it is for this reason, I suggest, that our cricket player may mistake one for the other at this distance. If he sensed both as flat, as he would sense the flatness of the disc two yards off, he certainly would not hesitate to pronounce both data *discs*. It is because both distant frontal shapes are *sensed* as *determinables*, not determinates, that he hesitates and may be deceived in some subsequent judgment based on those data. Now Mr. Price may be equipped with a sensory apparatus quite different from mine, such that he really does sense distant frontal shapes as all flat. But that is improbable, and it is more probable that we can come to an agreement by forgetting the numerous theories of flat *sensa* and looking these *sensa* straight in their frontal shapes. This procedure

seems to reveal some as determinate (those within stereoscopic range), others as determinable (those not within this range).

(4) Leave the disc at a distance and place the ball again two yards from the point of view. Compare frontal shapes. Mr. Price would report a definite dissimilarity, the proximate frontal shape being bulgy or hemispherical, the remote one flat. I report that, as in case (2) of our experiment, I cannot make a strict comparison at all. I am unable to compare determinate shape (the near hemispherical one) with determinable shape (the frontal shape of the remote disc). After all, comparisons must be "in kind," and there is a sort of "theory of types" involved in the description even of sensibles. All I can be strictly said to sense in this fourth situation is *some* frontal shape of the distant disc, but determinately none. And it is not feasible to compare this with the determinate bulge of the near-by ball. Furthermore, if Mr. Price is going to insist that the two frontal shapes are similar in case (2), where he said the near flat disc-datum was like the distant *flat* ball-datum, I shall insist with as much justification—i.e. not any whatsoever—that they are similar in this fourth case, where I might say the near bulgy ball-datum is like the distant *bulgy* disc-datum. I can *imagine* the remote frontal shape of the disc to be bulgy as readily as Mr. Price can *imagine* the remote frontal shape of the ball to be flat. But the attempted comparison in each case is really between a determinate and a determinable shape, and is consequently not feasible. Let us again call on the cricket player to help us settle this issue of the fourth stage of the experiment. Does he discover the bulgy frontal shape of the proximate datum (ball) to be like or unlike the frontal shape of the remote datum (disc)? He replies, "Being a cricket player, I am inclined to see that distant red patch on the green as bulgy, but, strictly speaking, at this distance I cannot tell . . ." And his confession of sensory inability coincides with mine.

Before passing on to the consideration of what probably induced Mr. Price to make this *faux pas*—and he makes remarkably few—I wish to clear up a small matter. It may be argued by some one that I should have said, "One cannot sense the frontal shapes of the distant ball and disc," instead of "One senses determinable frontal shapes in the case of the distant ball and disc". The difference is between not sensing any frontal shape at all, and sensing a determinable one. It must be admitted, I believe, that the difference is considerable. Since, however, Mr. Price's view of flat distant frontal shapes stands in need of revision on either interpretation, I shall not here go into this distinction. I shall merely state a preference for the expression "sensing determinables". I prefer this descriptive phrase because it seems to me I often do sense a colour-expanse having some, but determinately not any, frontal shape, as for instance when I look at the moon. I then sense a surface that is either flat or not flat. However, if some one insists that this is tantamount to asserting that I do *not* sense any frontal shape at all, I am not going to quarrel with him here. Below he will come across a parenthetical

comment on determinable distances as given for awareness, which may incline him to countenance the use of the adjective "determinable" in connection with other given characteristics, such as frontal shape.

Now as to why Mr. Price asserts the flatness and the "facing straight forward" of all data in the plane of maximum depth of the visual field. Perhaps on noticing the "fading out" of the bulginess or stereoscopic properties of data as the distance between them and the point of view increased, he inferred not only their flatness at a distance, but also a maximum visual depth beyond which they cannot go (pp. 218, 221). This plane of maximum depth is conceived by him almost as a kind of wall against which data become flattened as they impinge against it in their recession from the point of view. Now since this description describes nothing in my sensory experience, I must give another description. Instead of inferring that a remote frontal surface must be sensed as flat because certain protuberances or depressions are no longer sensible in it—and I suspect Mr. Price did *infer* this, not observe it—I examine the datum and notice an indeterminacy as to frontal shape. When I sense a frontal surface as flat and "facing forward," I invariably find that the datum it characterises is *within the stereoscopic zone*, not at its maximum depth. Indeed, *its being determinately flat and its facing this way or that—e.g. forward—are some of its stereoscopic values*, such that if the datum is removed to a non-stereoscopic depth, I simply cannot tell whether the frontal shape is flat or which way it faces. So when Mr. Price says that data become flat and face forward as they reach a non-stereoscopic depth, he is quite certainly using "flat" and "facing forward" in a peculiar sense—a sense in which he could *not* use them when he came to describing the flatness and facing forward of our cardboard disc two yards from his point of view, *i.e.*, when the flatness and the facing *are* stereoscopic. I beg Mr. Price to consider this point well. If he chooses to call "flat" and "facing forward" the sensed frontal shape of a cumulus cloud towering miles away above the horizon, then he should invent two corresponding words to describe the frontal shape of a penny in the palm of his hand and on which is he looking directly down. Of course, it is easy to *imagine* the frontal shape of the cloud as flat—or bulgy for that matter—but that would be engaging in the kind of "attribution" Mr. Price is so anxious to avoid.

With regard to the flattened-against-a-wall conception of data in the plane of maximum visual depth, Mr. Price seems to have made essentially the same mistake. He has mistaken determinable distances or "depths" for determinate. Again he seems to have argued in the following vein: because the moon, for example, is not sensed as nearer to or farther from the point of view than the stars, and because the stars are not sensed as nearer or farther away than the moon, moon and stars are sensed as equidistant from the point of view: hence a "plane of maximum depth". My own sensory experience responds favourably to the premises of this argument,

but not to the conclusion. The proposition that P is sensed as neither farther away nor nearer than Q might be true while the proposition that they are sensed as equidistant is false. No determinate distance-interval at all may be "given". I certainly do not *sense* star-data—i.e., star-data are not given to me—as exactly as far away from my point of view as the moon datum, however easy I may find it to imagine or judge this, even as it would be easy to imagine an inequality in their distances. But, barring such attribution, what I literally sense is an array of luminous data at indeterminate distances. (Obviously it would be false to say that one is aware of no distance-interval whatsoever, though it be true that no *determinate* distance be given. Hence the phrase "determinable distance". Compare the above phrase "determinable frontal shape".) My bare sensory experience is incapable of distinguishing "nearer" or "farther" or "equidistant" in this non-stereoscopic zone, since these determinate distance-values are there not given. The "givens" are indeterminate depths. Now this is not preparing the way for the assertion that it takes a conceptual schema or "synthesis" of the Kantian sort to spatialise data definitely. I mean rather that, just as determinate depths are sometimes *given* within the zone of stereoscopy, so determinable depths are sometimes *given* along with data lying outside that zone. This means, moreover, that there is a sensible (though indeterminate) "beyond" for any stereoscopic zone, whereas, according to Mr. Price, there can be no such beyond but only a limit, a flat plane of maximum depth, in which data come to an abrupt halt in their recession from the observer. He would describe my sensory experience of a hawk flying high in the sky as a flat brown datum lying and moving in the plane of a flat blue sense-datum. But, stare as I will, I sense only an indeterminate frontal shape of the expansive, ethereal sky-datum, as well as of the hawk-datum, such that I am unable to report, on the basis of sensation, that one *flat* datum is lying in the *flat* plane of another, or that the distance between them, or between them and my point of view, is precisely thus and so. They are cases of what might conveniently be called "sensory determinables" or "determinable sense-data".

Mr. Price may argue that if the sky actually were a concave shell painted blue and appropriately illuminated on the inner surface, then, plus a hawk painted on it, it would look exactly as it often now does *qua* atmosphere. I agree with him unreservedly. But are we to conclude that *therefore* the hawk and sky data are *sensed* as lying in the same flat plane? Certainly not. Even in the case of the shell or dome, if it were sufficiently lofty to simulate the sky, its frontal shape and distance, as those of the painted hawk, would be *sensible determinables* for the gazer below, not determinate sense-data. By merely sensing, he could not tell about the various distances and frontal shapes involved. The fact that a flat colour-patch "really" is painted on, and in the same flat or concave surface as, another colour-expanse, does not prove that, in conditions unfavourable to stereoscopy, an observer is bound to sense them as such. To assert

that it does would be like asserting that a ball, because really bulgy, must be sensed as such at an ultra-stereoscopic depth.

I would not have hen-pecked this issue of determinate and determinable sense-data so meticulously if I had not detected in it something of importance for the theory of knowledge in general. One of the major arguments of epistemological dualism or one of the main reasons for belittling the world as it appears in ordinary perception is that since things are determinately sensed as thus and so (e.g., the alleged equidistance from the point of view of heavenly bodies or the "flatness" of distant things) and since these things are otherwise known *not* to have the sensed characters (e.g., the stars are really at extremely diverse distances and the distant cricket ball is really bulgy, etc.), things cannot be as they appear to be in ordinary sense-perception. But if many of the so-called sensory determinates, conflicting with determinate characters otherwise known to belong to the object, are in fact *determinable* characteristics or sensed as determinables, then there is no conflict, or at least not nearly as extensive or implacable a conflict as is often supposed. Science, for example, may ascertain the real distances of the stars without being challenged by sense-perception. Where pure sense-perception reveals nothing, for example, about determinate (real) distances of objects lying in that spatially indeterminate zone into which the stereoscopic zone disintegrates and merges as distance increases, there could be no quarrel between science and sense-perception.

Besides certain distances and frontal shapes there are, I believe, other sensory determinables, such as for example profile, change, movement, surface-texture, perhaps even colour in a delicate sense. But it was not my aim in this paper to discuss all of these. In a concluding remark, I wish to draw the attention of the reader to another merit of the concept of determinable sense-data. If all data were perfectly determinate in all respects, error, in the sense of any kind of mistaken inference based on these data, would be a monstrosity. But where the data themselves are infected with indeterminacy or are often mere determinables—as I have tried to show—error acquires a natural status and can get naturally accounted for. The man who has to *grope among his data qua data* may be expected to make mistakes. When, during a bear hunt on an autumnal evening in the Himalayas, I came near to shooting down an Indian wood-cutter out of an oak, it was not because I sensed, in the meshes of that tree, a determinate sense-datum, "having just exactly this shade of colour and just this shape and no other". Indeed, had the frontal shape of that black figure in the branches against the western sky been "given" to me as *flat*, I should have wondered at the phenomenon and perhaps musingly punctured it with a bullet for practice, *knowing* that it could be no living thing.

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SPATIAL CHARACTERISTICS OF PHYSICAL OCCUPANTS.

IF I may be forgiven for reviving a topic which was not long ago under discussion in the pages of *MIND*, viz. some points in the theory of perception which Mr. H. H. Price puts forward in his recent book entitled *Perception*, I should like to point out here that an inevitable incompatibility or clashing of principles appears to manifest itself when the implications which flow from the interesting theory of Families of sense-data and their properties are contrasted with the final realism which the author accepts.

Mr. Price regards physical space as a *collective* characteristic belonging, not to sense-data in their individual capacity, but only to certain groups of sense-data which he calls the *nuclear* members of Families. It is these which enter as constituents into the formation of Standard Solids. He holds that sense-data, although they are themselves extended (*i.e.* in the case of the visual and tactual senses), and although they stand to one another in relations of extension within the same sense-field, do not individually occupy positions in physical space and are not themselves extended in it. The relations of all sense-data to physical space are not, however, the same. The non-nuclear members of Families have to be considered in this respect separately from the nuclear members. Non-nuclear data are those which are not perfectly constructible and which belong to some kind of distortion-series; and they are related to the Standard Solid towards which such distortion-series converge, not by spatial relations, but by the relation of Progressive Adjunction, which leads to *eventual* coincidence with the system of Standard Solids.

Without recapitulating the whole of Mr. Price's argument about this kind of relationship, it is sufficient for my present purpose simply to quote his statement that, "Non-nuclear sense-data are not in physical space".¹

The relation of nuclear sense-data to physical space is less easily stated. Here Mr. Price says that it is a misleading question to ask whether or not nuclear sense-data have positions in physical space, because "Physical Space is itself *defined* in terms of nuclear data".² Still, it is clear that no individual sense-datum, even if it is a nuclear one, has quite literally a position in physical space. The relation of sense-data to one another is that of Progressive Adjunction, and in the case of nuclear data, this relation is *constitutive* of physical space, existence in which henceforth characterises

¹ H. H. Price, *Perception*, p. 249.

² *Ibid.*, p. 252.

the nuclear groups of sense-data *collectively*. So it follows that, "Position in that space will be a *collective* characteristic belonging to a certain group of sense-data as a whole, and to nothing less".¹

Since the character of the material world only emerges with this system of nuclear groups of sense-data, it is towards collective characteristics that the whole theory of Families inevitably tends. The point I wish particularly to consider is the connection which this collective view of physical space has with the author's account of the constitution of the complete material thing. He says that the material thing arises from the conjunction of a family of sense-data with a physically occupied and causally characterised region which he calls the Physical Occupant; and this *conjunction* consists not merely in the fact that there exists a causal relation between the physical occupant and the family, but that there is *coincidence* between the two. Thus he says, "But in fact sense-data cohere together in families, and families are coincident with physical occupants"; and again, "A physical occupant . . . is *defined* as a causally characterised entity with which a family of sense-data is coincident".² This coincidence is essential; it is involved in the actuality of any piece of matter. As the author says, when we use such a word as 'table,' "We mean a certain sort of family *together with* the physical object which is coincident with it".³

But this 'coincidence,' which forms the central item in the theory of the constitution of the material thing, is extraordinarily difficult to conceive in any terms which are consistent with the theory of collective characteristics. For the physical occupant in isolation does not seem to be the sort of thing with which a family of sense-data (or, for that matter, anything else) *could* coincide. As Mr. Price says, "A pure physical object is something so shadowy that we can scarcely conceive of it at all."⁴ Yet in order that it may be possible for a family to coincide with it, must it not possess quite definite spatial characteristics? The characteristics of physical space, however, it cannot possess, for they are the unique possession of particular groups of sense-data considered as collective wholes. If individual sense-data have no independent positions in physical space, still less can shadowy physical occupants have them. But then, how can a group of sense-data, situated in physical space, *coincide* with a physical occupant which has no place at all in physical space, but is merely a *constituent* of something which has? Certain individual sense-data can be coincident with one another because they coexist in the same sense-field, although the extended character of that sense-field (or its sense-field-space) may not be the same thing as physical space. But individual sense-data, which are capable of coinciding with one another, exist within the same extended system. Here, however, we are speaking of a very different thing, *viz.*, of the coincidence of entities which are *not* included in the same

¹ H. H. Price, *Perception*, p. 251.

² *Ibid.*, p. 302.

³ *Ibid.*, p. 301.

⁴ *Ibid.*, p. 303.

extended system. We are saying that a group of sense-data, which exists in a space of its own (a space which is *solely* a collective characteristic), is coincident with a physical occupant which, *ex hypothesi*, cannot exist independently in this space, and does not seem to possess a space of its own. If it could be said that the physical occupant already and independently possessed a shape, a size and a position in physical space, then it would be clear what is meant by saying that a family of sense-data coincides with it. But then it could be no longer held that physical space is a collective characteristic.

But, is it possible to hold that, after all, physical space is not a collective characteristic of certain groups of sense-data alone, but only of the complete material thing? That is to say that, only when a physical occupant is causally conjoined with a family of sense-data does physical space appear at all? In that case, it might be said, it is only material things which exist in physical space. Sense-data do not and physical occupants do not. It only appears on analysis *as if* the family coincided with the physical occupant in physical space.

Such a theory is not convincing. The constructibility of sense-data is something which varies in degree. There is not a radical difference in kind between constructible and non-constructible groups of sense-data, the former kind being conjoined with a physical occupant and the latter kind not. For example, there are highly constructible groups of sense-data, such as mirages, which appear to exist in physical space and yet which are not coincident with physical occupants. Physical space is, as Mr. Price so clearly shows, a collective characteristic of certain peculiar groups of sense-data, and if this conclusion is consistently maintained it is impossible to see how such a group can be *coincident* with a physical occupant.

There are, however, other passages in Mr. Price's book which reveal the physical object, or occupant, as less shadowy than the above quotation would show. It is spoken of as occupying a 'region' and as possessing the characteristics of a 'substance'. Thus, "If the region has causal characteristics, it is a substance and not simply a region: and since causal characteristics are physical, it is a *physical* substance".¹ If we accept literally the implication that the physical occupant is, or occupies, a region in physical space, then the collective space of the families has simply been stolen and bestowed upon the physical occupant in order to supply its poverty and make the theory work—that is to say, to make the theory turn out to be a realist theory of a sufficiently literal type. The illuminating way in which the theory of families has been developing is thereby violently forced at the expense of consistency into line with the desired kind of realism. The latter demands that there *must* be a literally space-dwelling physical object; but the collective properties of families of sense-data do not tend to square with

¹ H. H. Price, *Perception*, p. 281.

this demand. We see the difficulty in the following sentence. "In order to describe it [the physical occupant] in any definite way, *e.g.* to attribute to it a particular shape or size or location, and a determinate sort of causal characteristics, we have to conceive it to be related to certain families, with whose members we are actually acquainted in sense."¹ But, in order to conceive it as related in any other way than causally to the family, we must first endow it with the family's spatial characteristics, and then the argument becomes circular.

The same difficulty in another form would seem to arise with regard to the *substantial* character of the physical occupant, only in this case the inconsistency is not so obvious. If the physical occupant is a substance, it must presumably possess the characteristic of enduring in the same time as that in which complete material things endure. Yet here again there would seem to be a clash between the characteristics of the physical occupant and the *collective* characteristics of families of sense-data. For it is not only spatial characteristics which are collective. "The characteristic of being an event in Nature," says Mr. Price, "like the characteristic of having a spatial position in it, is a collective characteristic which no sense-datum can possess, but only certain sorts of complexes of which sense-data are constituents."² This would exclude events in physical occupants from being events in Nature. But then it is the physical occupant which, according to the theory, is the sole causal agent in Nature. It is responsible for what Mr. Price calls Horizontal as well as Vertical causality; that is to say, it is causally efficacious in giving rise to those events in Nature which are specifically defined as being *collective*. Events which figure in the world of collective things are causally referable to physical occupants which themselves have no place in the world of collective things. It is not easy to see how physical occupants can be held to endure in that time in which collective events occur (as to be 'substances' in the ordinary sense of the term they must do), and yet that it should be impossible for such collective events to occur to *them*. Yet if they do not endure in the time in which complete material things endure, must they not renounce their claim to be physical substances?

The complete discussion of this point would occupy too much space. What I wish to point out is that the theory of a literally physical partner capable of coinciding spatially with a family of sense-data and enduring in the physical time of material things is not consistent with the illuminating theory of collective characteristics which Mr. Price has developed. I am not suggesting that this theory of collective characteristics is wrong, but only that it would seem to require a conception of the physical occupant which is less literally realistic than that which Mr. Price adopts.

G. N. M. TYRRELL.

¹ H. H. Price, *Perception*, p. 303.

² *Ibid.*, p. 319.

V.—CRITICAL NOTICES.

Collected Papers of Charles Sanders Peirce. Edited by CHARLES HARTSHORNE and PAUL WEISS. Cambridge (Mass.): Harvard University Press. (London: Humphrey Milford.) Volume I.: *Principles of Philosophy*, 1931. Pp. xvi + 393. \$5: 21s. Volume II.: *Elements of Logic*, 1932. Pp. xii + 535. \$6: 25s. Volume III.: *Exact Logic*, 1933. Pp. xiv + 433. \$5: 21s. Volume IV.: *The Simplest Mathematics*, 1933. Pp. x + 601. \$6: 25s.

ON the death of C. S. Peirce in 1914, at the age of 74, his unpublished manuscripts came into the care of the Department of Philosophy of Harvard University. A selection of these, together with his published papers (about 75 in number), his book-reviews, and his letters, are now being published by the Harvard University Press in some ten volumes, which are appearing at the rate of one about every eight months. Volume I. contains the outline of Peirce's system of philosophy, together with his remarks on "General Historical Orientation", on the classification of the sciences, and on ethics. Volume II. contains his work on what would be called general or philosophical logic—the function of logic as a study, the theory of signs, traditional syllogistic logic and Peirce's important writings on probability and induction. Volume III. contains his published papers on formal or symbolic logic, which include the work on the logic of relatives which has won him an important place in the history of the subject. Volume IV. contains his unpublished papers on formal logic, and on the foundations of mathematics: these include Peirce's scheme of "existential graphs", which he took to be his "*chef d'œuvre*". Volume V. contains all the papers which are relevant to the claim made on his behalf by James to be considered the founder of pragmatism: they show that his "pragmatism" differed in many ways from the doctrines of James or of Schiller.¹ Volume VI., we are told, will be concerned with metaphysics; and the remaining volumes will contain his writings on psychology and physics (Peirce was for some time on the staff of the United States Coast Survey, and many of the papers deal with the determination of gravity by pendulum experiments), his reviews, letters and biography. When

¹ Volume V: *Pragmatism and Pragmaticism*, 1934. Pp. xii + 455. \$5: 21s., has only just appeared; though I make some references to it, I am not including it in the scope of my review.

these remaining volumes are published we shall have as thorough a knowledge as possible of what Dewey rightly calls " the most original philosophical mind " produced by America.

To judge from the accounts given of the mass and confusion of Peirce's unpublished papers, the editing of the " Collected Papers " must have been about as difficult as any task of editing can be ; and Dr. Hartshorne and Dr. Weiss have earned our gratitude by the success with which they (with the assistance of members of the Harvard Philosophical Department and others) have carried out their task. The manuscripts left by Peirce were in all stages of completeness : they included drafts on the same subject written at different times (sometimes the date could not be discovered or inferred), and the editors have had to select the material for the volumes so as to include most of the good passages which he wrote. Thus a certain amount of repetition has been unavoidable. Moreover, few of Peirce's papers deal with one point only : he was a great enough philosopher to see that most philosophical problems are connected with other philosophical problems ; so he treats of the same subject (*e.g.*, his doctrine of signs) in a great number of contexts. This has made the arrangement of the papers exceedingly difficult ; and I imagine that the editors' difficulty was increased by the practical necessity for publishing the volumes at different times. The first volume contains papers on a strange variety of subjects whose bond of union seems to be that they are not specifically concerned with logic ; but the justification for this is presumably that the volume appearing first had to convey a general impression of Peirce's manner of thinking, and in this it is very successful. I was distressed to find only three of the famous six articles on " Illustrations of the Logic of Science " which originally appeared in *Popular Science Monthly* (1877-78) and were republished in *Chance Love and Logic* (1923), collected in the discussion of induction in Volume II. But I realise that it was necessary to save two of these articles for Volume V. if this volume was to include everything that Peirce wrote germane to pragmatism. If all the papers could have been published simultaneously, another classification might have been possible and desirable : granted successive publication, the editors have probably made the best of a very nasty job, and have shown considerable patience and ingenuity in fitting together the mosaic.

But there is one feature of the edition which I do criticise : the biography is postponed till a late volume, so that we know very few of the facts of Peirce's life. This is not merely my vulgar curiosity (although a person presented with a million words of a philosopher has surely the right to have even this satisfied). For anyone reading Peirce's manuscripts, where very " hard-headed " statements and reasonable argumentation are sprinkled and sometimes swamped with fantastic suggestions, will feel how much his work would have gained had he been subjected to the continual criticism of

intelligent students. When Peirce says, for example, that though "I am a determined foe to no innocent number; I respect and esteem them all in their several ways", yet "I am forced to confess to a leaning to the number Three in philosophy" (1.355), one feels that he should have heard the ribald laughter which seems fitting to that remark. And though his style is most obscure and involved when he is treating of what he thinks are the most difficult matters, at other places he writes so well that one cannot believe that he could not have improved the obscure parts if he had been writing for an audience. For both these reasons, therefore, I think it was a great misfortune that Peirce taught at a university for a few years only; and my curiosity is aroused as to why his great gifts were not thus used. If, as is rumoured, the reason for this had nothing to do with intellectual capacity, we should have been told this: otherwise one is tempted either to criticise Peirce for an inability to adapt himself to the needs of students or to condemn American universities for not having employed a most stimulating teacher. Both these criticisms are probably beside the mark; and I hope that the biography, when it appears, will dispel what is to me a mystery.

It should be mentioned that the editors have done the routine work of editing extremely well. Every paragraph is numbered (3.647 means the 647th paragraph of Volume III.), and the volumes are excellently indexed and cross-referenced. In my considerable use of these references I have found only three or four mistakes: the only one worth mentioning is "Keynes, J. M." for "Keynes, J. N." in the indexes to Volumes III. and IV. Helpful footnotes have been added, and in Volume III. (which contains the published papers on symbolic logic) a supplementary table of contents has been prepared to enable a reader used to modern terminology to find his way about the book. I have heard it complained that the editors have not paid sufficient attention to chronological order in their arrangement of the unpublished manuscripts. But most of Peirce's views, even the inconsistent ones, he held all his life: elements of his later doctrines are to be found in his earliest papers; and I think the editors were right in classifying according to subject instead of according to date. The date of every paragraph that the editors know can be ascertained (though one sometimes has to turn over several pages to find it). I miss the dates on the admirable photographs serving as frontispieces to Volumes I. and V.: the former photograph shows a resemblance to W. K. Clifford, whom I fancy Peirce was like in many ways.

If the editing of the "Collected Papers" must have been exceedingly difficult to the editors, their reviewing is not easy. Even were it desirable, it would not be within my powers to pass systematically in review the whole contents of these four volumes, expounding and criticising all the topics in a manner worthy of MIND. So, after some very short remarks on Peirce's general philosophy and on his

work on symbolic logic, I shall treat at more length the two most important parts of Peirce's work on general logic—his theory of signs and his theory of probability and induction. These doctrines display very well Peirce's weakness and strength as a logician.

Peirce's Philosophy.—Only a selection of papers dealing with Peirce's metaphysics has been published in Volume I.; the remainder will appear in Volume VI.: so we are not yet in a position to pass judgement upon Peirce's system. Peirce classifies the things which are in any way present to the mind (*phanerons*) into three categories, which he calls by the non-committal names of Firstness, Secondness, Thirdness. The category of Firstness includes "qualities of feeling, such as the colour of magenta, the sound of a railway whistle, the taste of quinine, the quality of the emotion upon contemplating a fine mathematical demonstration, the quality of feeling of love, etc." (1.304). These are all possibilities, not actualities: they need not "inhere" in a subject: they are the *universalia ante res* of the scholastic realists (Peirce greatly admired Duns Scotus). Secondness comprises the actual facts of the world—the *hic et nunc* of things. (Peirce never distinguished between particular existents and facts.) Secondness comes before the mind as the "element of struggle": it is involved in the consciousness of otherness, of action and reaction, of volition. Thirdness includes the meaning of signs, the conception of general laws, "infinity, continuity, diffusion, growth, and intelligence" (1.340). It is the category that is concerned with connecting, and it is involved in all reflective thought. Peirce's three categories start by being phenomenological categories, and perhaps because of this his metaphysics of nature, with its struggle and something analogous to thought, seems very anthropomorphic. Peirce is thoroughly aware of this; and defends himself boldly by saying that "other things being equal, an anthropomorphic conception, whether it makes the best nucleus for a scientific working hypothesis or not, is far more likely to be approximately true than one that is not anthropomorphic" (5.47 n.).

Volume I. includes a short section on the Normative Sciences: most of what Peirce wrote on this subject was in connection with pragmatism and logic and occurs in Volumes II. and V. "The fundamental problem of ethics is, What am I prepared deliberately to accept as the statement of what I want to do? . . . Now logic is a study of the means of attaining the end of thought. It cannot solve that problem until it clearly knows what that end is. . . . It is, therefore, impossible to be thoroughly and rationally logical except upon an ethical basis" (2.198). But Peirce admits that a logician must be on his guard against being "over-influenced by the moral spirit"; and declares that what was "at the root of the barbarism of the Plantaganet period and paralysed the awakening of science from the days of Roger Bacon to those of Francis Bacon" was "the exaggerated interest men took in matters of vital importance" (1.675). For "all sensible talk about vitally important

topics must be commonplace, all reasoning about them unsound, and all study of them narrow and sordid" (1.677).

Symbolic Logic.—It would be impossible to give an adequate account of Peirce's published contributions to symbolic or "exact" logic (as he liked to call it) much more shortly than is given in the admirable 28 pages devoted to Peirce in Lewis's *Survey of Symbolic Logic*. I will only mention, therefore, the four things which class Peirce with Boole as co-founders of mathematical logic as it is understood to-day: (1) the use of *inclusion* as the most important notion, (2) the application of the algebra of logic to propositions by limiting the values of the variables to 0 and 1, (3) the purification of the algebra of extraneous arithmetical elements (Peirce's progress in this direction can be seen by comparing the paper of 1870, in which logarithms and differentials occur, with those of 1882-85), and (4) the development of a logic of relatives, *i.e.*, of propositional functions of more than one variable. In this connection Peirce developed the notion of variable, of definition in extension and of the relations between $(\exists x) : (y) \cdot \phi(x, y)$ and $(y) : (\exists x) \cdot \phi(x, y)$ almost in the form (though with quite a different notation) to be found in *Principia Mathematica* thirty years later. The great defect of Peirce's work is that he hardly ever proves anything; compared with Schröder's or Whitehead and Russell's work, his papers seem merely preliminary sketches. But his originality and subtlety are manifest throughout, and his shrewd comments on, for example, what is now known as "material implication" (which he refers back to Philo of Megara) are well worth study to-day. He was also thoroughly imbued with the modern conception of exact logic as a calculus.

There is one point which I should like to mention, because I have not seen it referred to by other commentators. It is surprising that the inventor (after De Morgan) of the Logic of Relatives would not admit of any unanalysable relations of a higher order than the third. I cannot help thinking that his main reason for this intolerance was that his metaphysics did not allow any place for Fourthness. But he gives a formal reason for it which is worth examining: I give it in my own words. Every tetradic relationship can be analysed in terms of two triadic relationships: thus "A sells B to C for the price D" may be analysed into "There is a transaction E such that A enters into E with C and such that E is the sale of B for the price D" (1.363).¹ Similarly, relationships of higher orders can be analysed into triadic relationships by the introduction of one or more extra terms. The objection is that this procedure, however symbolically convenient, cannot be justified as a logical analysis in most cases: the meaning of "transaction" presupposes an understanding of the original tetradic relationship which is supposed to be explained by it. The extraordinary thing is that Peirce uses this very argument against A. B. Kempe, who formally analysed triadic relationships in exactly the same way as that in which Peirce analyses

¹ On l. 8 of p. 188 the "C" and "B" are transposed.

tetradic relationships (3.423). Peirce cannot have it both ways. If the question is that of representing propositions in a diagram of spots connected by lines, only lines connecting two spots (corresponding to dyadic relations) are required, and Peirce's triadic relations are superfluous. But, as Peirce says, "the difference between setting down spots in a diagram to represent recognised objects, and making new spots for the creations of logical thought, is huge" (3.424); and if we are not permitted to do this latter, Peirce's elimination of tetradic and higher relations is impossible.

In Volume IV., besides an interesting anticipation of Sheffer in a paper of about 1880, "A Boolean Algebra with One Constant," there are a number of papers and notes upon the foundations of mathematics which I think repay study. Peirce was well aware of the importance of Cantor's work on infinite numbers; but as he observes, "there are in Cantor's exposition of his ordinal numbers several points which will give the unmathematical student difficulty, not because he lacks intelligence, but because he thinks so exactly as to see the difficulties, while not being sufficiently acquainted with the subtleties of mathematics he is unable to solve them" (4.677). Now that the exact meaning of the mathematical subtleties is being doubted by the finitists, Peirce's acute commentary is not at all out of date. In many places Peirce discusses the question as to whether cardinal number is logically prior to ordinal number or *vice versa*, and decides in favour of ordinal number: here he is in agreement with the modern revolt against the Cantor-Frege-Russell orthodoxy. In this connection it may be noted that in his treatment of the general proposition in a logical sum and logical product symbolism, Peirce emphasises that general propositions are only similar to a sum or to a product; "they are not strictly of that nature, because the individuals of the universe may be innumerable" (3.393). Readers of Peirce's papers will find that not only did he discover independently (and sometimes first) many famous theorems in the theory of finite and infinite number, but that he was more conscious of the proper philosophical difficulties than most of his contemporaries. It is here as much as in any other part of his work that I regret that he did not publish a systematic treatise.

A great part of Volume IV. is devoted to Peirce's system of Existential Graphs, a form of logical diagram which Peirce considered his "*chef d'œuvre*" and the purpose of which is "to study the workings of necessary inference" by "representing diagrammatically any possible set of premisses, this diagram to be such that we can observe the transformation of these premisses into the conclusion by a series of steps each of the utmost possible simplicity" (4.429). I regret that I have not been able to study the system sufficiently to venture an opinion upon its importance, but I have seen enough to commend its examination to those interested in "exact logic".

PEIRCE ON SIGNS.

The papers of Peirce upon the theory of signs which the editors have thought worth preserving are to be mostly found in Volume II., Book II., and Volume IV., Book II. For the most part these very short papers have not been previously published, they vary in date between 1867 and 1910, they are deeply interfused with Peirce's metaphysic, and they are not completely consistent either in doctrine or in terminology. The editors inform us, moreover, that the letters to Lady Welby of 1906, which have not yet been published, contain certain complications of the doctrine and divide signs into 66 classes instead of the 10 classes of the papers in these earlier volumes. So I shall only be able to discuss what seem to me the main features of Peirce's account, and I shall do this independently of his metaphysical presuppositions.

Peirce defines what he means by "sign" in different ways in different places: the definition he gives in Baldwin's *Dictionary* is typical:—"Anything which determines something else (its *interpretant*) to refer to an object to which itself refers (its *object*) in the same way, the interpretant becoming in turn a sign, and so on *ad infinitum*" (2.303: similar definitions are given in 1.541, 2.242 and 2.274). Let us, to begin with, ignore all after the first comma and consider what Peirce means by "determine" and by "refer".

Though he nowhere explicitly discusses the meaning or meanings of determination, I think that he is meaning here and in similar contexts "causally determine". If this is so, Peirce is thinking of a sign from the point of view of its hearer or reader or interpreter: the sign is something given in advance of its interpretation and acts upon its interpreter. Peirce is not thinking of a sign as essentially something made by the speaker or writer in order to signify: he thinks of it as something whose significance is to be interpreted. Since every sign made or used by a person is intended to act as a sign to some interpreter, either himself or some one else (see 4.551), Peirce's emphasis on the rôle of the hearer would be unexceptionable if it did not, I think, lead him into neglecting sometimes the equal importance of the speaker.

There is more difficulty about Peirce's use of the phrase "refer to". Sometimes he uses it as synonymous with "being determined by". In one place he goes so far as to define "a sign, stretching that word to its widest limits, as anything which, being determined by an object, determines an interpretation to determination, through it, by the same object" (4.531). If "determination" means "causal determination", Peirce's "sign" would here be similar to Ogden and Richards' "stimulus similar to some part of an original stimulus and sufficient to call up an excitation similar to that caused by the original stimulus" (*The Meaning of Meaning*, p. 140). I will call a theory which describes the functioning of signs purely in causal terms without bringing in any intention to use a sign in a particular

way, a "naturalistic" theory of signs. It is, of course, held by materialists, who would eliminate the specifically mental element everywhere, but it would also be held by dualists who wished to retain only sensation and feeling as mental elements and to analyse thought causally. A great deal of Peirce's writing is consistent with his taking a naturalistic or, indeed, a behaviouristic, view of signs. When he has to speak of "a mind" in connection with signs, he frequently adds "or a Quasi-mind" (4.550, see 2.274, 1.540), and suggests that it is only out of consideration for the weakness of his readers that he brings in human minds at all. And some of the passages in Peirce's metaphysics would agree with Firstness, Secondness and Thirdness being considered as materialistic categories.

Nevertheless I do not think that a naturalistic theory of signs is adopted by Peirce. His metaphysics on the whole is not materialistic: indeed, it may with more reason be criticised as too anthropomorphic. Moreover, his whole account of signs is saturated with words like "represent", "stand for", which clearly presuppose the intention of a mind. In Baldwin's *Dictionary* Peirce defines "to represent" as "to be in such a relation to another that for certain purposes it is treated by that mind as if it were that other" (2.273). So I think that the sense in which the sign refers to its object is that it is intended by a mind to stand for that object, this relation being the essential and peculiar characteristic of symbolic thought. In this sense it is nonsense to talk of a sign referring to its object without an allusion, explicit or implicit, to the mind which is doing the referring. "Sign" is usually used in some such sense as this by logicians, and I shall call it the "logical" theory of signs. Though Peirce sometimes expresses himself in a naturalistic way, I think that fundamentally he held a form of the logical theory.

That he did so is also supported by his doctrine, difficult to interpret naturalistically, that the functioning of a sign requires the creation of a new sign, its interpretant, which refers to the object in the same way as that in which the original sign referred to it. The sort of situation of which he is thinking is where I hear and understand a sentence spoken by you with the intention of communicating some fact to me. Then, if the communication is successful, the fact to which I take the sentence as I hear it to refer is the same as the fact to which you took the sentence as you spoke it to refer. Whether or not the actual sign used by you is the same as the actual sign used by me depends on whether by "sentence" is meant the physical event or the set of private sense-data (and also upon one's view as to the relation of sense-data to physical events); but these are subsidiary questions: my understanding the sign used by me is certainly different from your understanding the sign used by you, and it is of this that Peirce was thinking when he said that the former (the interpretant) was different from but determined by the latter.

So far, so good. But Peirce insists at the end of his definition of "sign" that the second sign (the interpretant) determines a third

sign which in its turn determines a fourth sign, and so on *ad infinitum*. If "determines" is taken categorically, this would mean that in every case of the use of signs there was an infinite series of signs which were interpretants of the sign next earlier in the series. There is no reason to suppose that this occurs. For even if it were necessary in Peirce's metaphysical system for every interpretant to determine another interpretant, there is no reason, so far as I can see, why the interpretant of the interpretant should not be the original sign itself. But in fact Peirce makes it clear in many places that by "to determine" in these contexts he only means "to be capable of determining" (see 2.274, 1.542). So all the infinite series comes to is that there may be an infinite number of signs which might function as interpretants of one another. This is harmless enough: perhaps it is a bad way of stating the important fact, which is the basis of the logical theory of signs, that any sign can be used in place of any other sign if the interpreter uses them with the same meaning.

Peirce has other infinite series of signs which are equally harmless if his statements are taken merely as statements of logical possibility. In 1.339 he says that the object of the sign can be nothing but a sign of which the first sign is the interpretant: this only makes sense, I think, if it means that the object can be used as the sign of another object. In 2.274 he says that the relation of the original sign to its object is an object of which the original interpretant is a sign. If this means that I can think of my thinking of an object by means of a sign, and that I can then think of this thinking, and so on, well and good: if it means that in every actual case I do think of my thinking, and think of my thinking of my thinking, etc., it is fantastic.

Indeed in his definitions of "sign" in 2.242 and 2.274, Peirce only speaks of the triadic relation of the sign to the object and the interpretant as being "capable of determining the interpretant" or as "determining a possible interpretant". He adds that "while no [Sign] actually functions as such until it actually determines an Interpretant, yet it becomes a [Sign] as soon as it is fully capable of doing this; and its Representative Quality is not necessarily dependent upon its ever actually determining an Interpretant, nor even upon its actually having an Object" (2.275). But this would deprive "sign" of every vestige of distinctive meaning: if a sign need neither stand for an object nor signify for a person, there is no point in using the notion at all. Anything can be used as a sign for anything: Peirce's discussion and classification is only of value if he is discussing those signs which are actually used by speakers or writers to communicate their meaning.

In Volume II. Peirce makes a tenfold classification of signs which is based upon three fundamental trichotomies. I shall not discuss this tenfold classification, because the system all hangs on the three fundamental trichotomies which I think are themselves open

to criticism. The three trichotomies depend upon dividing signs according to three principles each of which yields three sorts of sign. The three principles of division are in the nature of the sign itself, in the relation of the sign to its object, and in what the sign is taken to represent. I will consider the three trichotomies in turn.

The Sign Itself.—According to Peirce, a sign is (1) a mere quality, a *Qualisign* or *Tone*, (2) an actual existent, a *Sinsign* or *Token*, or (3) a general law, a *Legisign* or *Type*. As examples of the three kinds he gives (1) a tone of voice (an "indefinitely significant character": 4.537), (2) a particular word "the" on a page, (3) the written word "the" in the English language of which the particular "the"s are instances (2.246). It should be noted that Peirce's distinction between his sinsign or token and his legisign or type is a distinction depending purely upon the nature of the sign itself and in no way upon what the sign is taken to mean: thus the legisign "the" is the class of written words having a certain physical similarity, not the class of written words having the same meaning. (Ramsey, who introduced Peirce's words "type" and "token" into modern logical discussion, used "type" in the first sense as applied to a propositional sign and in the second sense as applied to a proposition: *Foundations of Mathematics*, p. 274). Using "legisign" then for the class of sinsigns having a certain character in common, or (if you like) as the common character, I cannot distinguish it from a qualisign. For the specific quality of voice by which I recognise a friend (a qualisign) must be embodied in some particular event which has that quality in order for it to serve as a sign, just as the legisign "the" has to be embodied in its instance. Peirce admits this, but says that in the former case "the embodiment has nothing to do with its character as a sign" (2.244). This seems to me like saying that the existence of a patch in my visual field has nothing to do with my seeing it as red: I could not see it as red unless there were an *it* to see; I cannot take the tone of my friend's voice to be a sign of him unless the voice exists.

Peirce's trichotomy reduces then to a dichotomy of signs into qualisigns or legisigns and sinsigns. And there can never be one of the former without one of the latter. Can there be a sinsign without a corresponding legisign? that is, can an individual particular or event function as a sign except by virtue of having some property in common with other signs? I think that the answer to this question is: In a private system of signs, yes, for the thing itself can be used as a sign; but in a public system of signs (of the sort which Peirce is considering), no. For in order for me to be able to interpret the sign used by you to mean the object you mean by it, I must have reason to believe that you are using the sign with that meaning, and I can only do this if I have reason to believe that you always use a sign of a particular sort to have the meaning in question. I shall not be able to interpret your sinsign unless I regard the sign as a legisign as well.

It is interesting to note that the same consequence follows if the working of signs is considered purely causally according to the naturalistic theory. For a cause is an event having some property: the event's having that property causes another event to have some property; so on a naturalistic theory both a *sinsign* and a *legisign* are involved in all sign situations.

The upshot of this is, it seems to me, that while in certain connections it may be important to distinguish the sign as *legisign* or type from the sign as *sinsign* or token, yet any actual object or event which is a sign must function both as type and as token.

The Relation of the Sign to its Object.—The trichotomy under this heading is the one to which Peirce attaches the most importance: (1) An *icon* is a sign which represents its object by virtue of having some character in common with the object: the colour of a colour-card as representing the colour of the object which it resembles is an icon, and a map as representing spatial relations is an icon. (2) An *index* is a sign which represents its object by the latter's having some direct action on the former: an example of an index is a hole in a wall taken as a sign of the passage of a bullet (2.304). (3) A *symbol* is a sign which only represents its object by a convention or law that it is to be interpreted in that way: any word or sentence is a symbol. Peirce describes the distinction also in terms of the reducibility of the fundamental triadic relation involved in the use of signs: I think that the essential point at issue can be expressed more simply by saying that in the case of symbols there is no relation between the sign and its object which by itself determines its interpretation, whereas with icons and indices there is such a relation. In the case of an icon this relation is one of similarity in respect of some property; in the case of an index it is some relation such as causality holding directly between the sign and its object.

Now Peirce does not want to make merely the obvious point that some things used as signs resemble their objects, and that other things used as signs have been directly caused by their objects: he wants to contend that these facts are relevant to their functioning as signs. And I do not think that he makes any good case for this. In some of his reasoning he seems to confuse two questions which he is usually at pains to keep distinct—the psychological question of the historical origin of our use of signs, and the logical question of the correct analysis of our use of them at present. It may quite well be that the signs first used in the history of language were signs which had a direct causal relation to their objects (*e.g.*, gestures of touching objects) or were signs which resembled their objects (*e.g.*, imitative gestures representing movements), but from the logical point of view in neither case were these gestures in themselves signs: they only became signs when they were used for the purpose of communication or understood as standing for their objects. Peirce himself puts the case against the separate existence of indices when he says: "When a driver to

attract the attention of a foot passenger and cause him to save himself, calls out 'Hi!' so far as this is a significant word it is . . . something more than an index; but so far as it is simply intended to act upon the hearer's nervous system and to rouse him to get out of the way, it is an index, because it is meant to put him in real connection with the object, which is his situation relative to the approaching horse" (2.287). But in the latter case the "Hi!" is not a sign at all; it is only a stimulus used for the purpose of affecting the hearer's body, not his mind. When the "Hi!" is a sign, it must be understood by the hearer to refer to the presence of danger. And the case is the same with icons. When I use a sign which resembles its object, as when I choose the cloth for a suit from a pattern book, I send the marked pattern to my tailor with the intention that he shall understand that I want him to make me a suit of cloth of a similar pattern, but my expectation that he will understand this by my marking the pattern is based upon the existence of a well-understood convention between tailors and their customers. As for maps, which are Peirce's favourite examples of icons, we all know how difficult it is to explain a route by means of a map to a person initially entirely ignorant of the principles of map-reading.

Peirce's indices and icons are supposed to be signs which signify independently of their use as signs: I find this notion self-contradictory. When Peirce defines a symbol as opposed to an icon or an index as "a sign which would lose the character which renders it a sign if there were no interpretant", and says that a symbol "signifies what it does only by virtue of its being understood to have that signification" (2.304), he is saying things which seem to me true of all signs. Peirce's indices and icons are similar to Stout's "suggestive signs" and "substitute signs" (*Analytical Psychology*, Vol. II., p. 193). Stout is surely right in contending that these sorts of signs are useful in calling up or in manipulating "expressive signs", but that without reference to these they are meaningless.

Of course, the acceptance of a naturalistic theory of signs would justify the attempt to divide signs in their relation to their objects on some such lines as Peirce's trichotomy. But I think that a naturalist would be very simple-minded to attach much importance to Peirce's division. For the essential thing in the functioning of a sign to a naturalist is its "sufficiency to call up an excitation similar to that caused by the original stimulus", and a resemblance or causal connection of the sign to the original stimulus goes only a very little way to explaining an efficacy due almost entirely to an acquired habit.

In his discussion of icons and indices Peirce confuses the matter, I think, by his contentions that logical diagrams have an iconical character and demonstrative symbols (logically proper names) an indexical character. With regard to logical diagrams Peirce says a great many very interesting things, and his contention that the

process of deduction consists in observing the properties of and making *Gedankenexperimente* upon diagrams contains important elements of truth. And when he says that "the arrangement of the words in the sentence, for instance, must serve as [an Icon], in order that the sentence may be understood" (4.544), he is approaching Wittgenstein's doctrine that a sign must have a "logical form of representation" in common with its object. But it is quite a different thing to say, as Wittgenstein would say (*Tractatus Logico-Philosophicus*, 2-27 *et seq.* 4-04), that a certain feature of an object (e.g., its "multiplicity") can only be "shown" by the sign having something in common with the object, and to say (what I take the Icon doctrine to say) that if a mark has a certain similarity to its object, then it is a sign for that object. With regard to demonstrative symbols like "This" in "This is a pencil", Peirce contends that I cannot understand the sentence unless there is a real connection between the "This" and the pencil pointed at which would make the sign an index. But his argument (2.287) only goes to show that there must be a real connection between the mind of the interpreter and the object, which is an entirely different thing. The distinction that is important for the theory of signs between a word like "This" and a word like "pencil" in the sentence is surely that the former is used for symbolic reference in a different way from the latter, not that in itself it has a relation to its object independent of the conventions of the English language. Peirce has thrown distinctions into his second trichotomy which should have been made within the field of his third trichotomy.

What the Sign is taken to mean.—I have criticised Peirce for making distinctions in his first and second trichotomies which have no importance for the theory of signs: my criticism here is that his division is woefully inadequate in distinguishing the sorts of things for which signs are taken to stand. He makes a division into (1) a *Rheme* (or *Seme*), a "sign of qualitative possibility", (2) a *Dicisign* (or *PHEME*), a "sign of actual existence", and (3) an *Argument* (or *Delome*), a "sign of law" (2.250; see also 4.538). There is not much difficulty about rhemes: they are signs for universals and correspond almost exactly to Russell's "propositional functions" (when this expression is used for the sign). But about dicisign there is a great difficulty in relating the various things Peirce has to say: his definition of a "dicisign" as a "sign of actual existence" would seem to make particulars the objects of dicisigns; but he analyses "the essential nature" of a dicisign as "the kind of sign that conveys information" (2.309), and says that "the readiest characteristic showing whether a sign is a Dicisign or not is that a Dicisign is either true or false, but does not directly furnish reasons for its being so" (2.310). This would make the dicisign the sign of a proposition (or perhaps of a proposition not directly known to be either true or false); and in Peirce's considerable discussion of propositions he uses dicisign almost as a synonym for

sentence expressing a proposition. And in other places (e.g. 2.260) he calls the sign of a fact a *dicisign*. There is also uncertainty as to his use of "argument". Sometimes he means by this merely the sign for a proposition stating a law, in which case it would not seem to be essentially different from a *dicisign* (especially since Peirce sometimes maintains that all propositions are of the form of laws: 2.356); but frequently he means by "argument" an inference from premisses to conclusion, in which case it seems to me wrong to call the whole made up of the sentences expressing it a sign: the separate sentences are signs, but the inference is the passage of thought produced by understanding the separate signs but not itself represented by a sign. Though Peirce is right in making a classification of signs in accordance with what they are understood to represent, he does not do justice to the very many different ways signs stand for their objects. His treatment of demonstrative symbols and signs expressing logical form under his second trichotomy has impoverished his third classification, which seems to be the only fundamental one from a logical point of view.

In concluding my somewhat destructive criticism of the basis of Peirce's theory of signs, I ought to emphasise that other interpretations could be given of parts of the theory which may be more in accordance with his thought. The difficulty of a commentator is that the papers on signs are fragmentary, and their contents not mutually consistent.

PEIRCE ON PROBABILITY AND INDUCTION.

The papers on the "logic of discovery" or "ampliative reasoning" that the editors have collected in Volume II. have for the most part been previously published. They consist of (1) three out of the famous six articles, "Illustrations of the Logic of Science", first published in *Popular Science Monthly* (1877-78) and reprinted in *Chance Love and Logic* (1923), (2) "A Theory of Probable Inference" published in the *Johns Hopkins Studies in Logic* (1883), (3) articles in Baldwin's *Dictionary of Philosophy and Psychology* (1901 and 1911), (4) some 1905 notes on the varieties and validity of induction, and (5) an unpublished note of 1910 on the Doctrine of Chances. These last two items throw new light on Peirce's views; so, although all but 26 pages of Peirce's work on probable and inductive reasoning have been previously published in fairly accessible forms, this review seems the place to give a critical account of the part of Peirce's work on general logic which I take to be of most permanent value.

Induction is, for Peirce as for every other logician, a species of inference; and inference for him is the process by which the mind passes from believing one proposition (or set of propositions) to believing another, the process of passage being "according to a general habit of thought" which the reasoner "approves as conducive to true

knowledge" (2.773). Without this conscious approval the passage of thought would not be inference; but apart from this Peirce always treats inference as a psychological (or even physiological: see 3.160) process. The validity of the inference, for Peirce, is "purely one of fact and not of thinking" (5.365); it depends simply on whether or not the habit of thought leads from true premisses to true conclusions. The same criterion of validity is applicable both to a habit of inference and to a calculating machine. "Say, if you like, that thinking has everything to do with the life of reasoning; I still insist that it has nothing to do with the logical criticism, which is equally applicable to the machine's performance and to the man's" (2.59). The habit of mind governing an inference "may be formulated in a proposition whose truth depends on the validity of the inferences which the habit determines" (5.367): such a proposition Peirce calls a guiding or leading principle or precept of inference; the leading principle is true because inferences made according to it are valid, and not *vice versa*.

In demonstrative inference the leading principle is known to be logically necessary, *i.e.* is known to be true by "a person who knows no facts, though perfectly *au fait* at reasoning and well-acquainted with the words involved" (4.67). It is logically impossible for the premisses of a demonstrative inference to be true but the conclusion false. Peirce calls demonstrative inference "explicative, analytic, or deductive": he holds that such inference, however useful it may be, can never "increase our real knowledge" (2.680). I shall not discuss this difficult question, nor Peirce's interesting views as to the diagrammatic method by which deduction has to be carried out, which has analogies with the operational methods of contemporary logicians.

The form of inference usually contrasted with demonstrative inference is problematic or probable inference; but if in the latter is included every inference in which probability is concerned in the conclusion, then some of these inferences are demonstrative and are called by Peirce "probable deductions". I think it is worth while classifying these "probable deductions" according to a principle which Peirce does not use. There are, first, those in which no rules of probability are explicitly involved: for example, from my knowledge that $p/h = \frac{1}{2}$ and my knowledge that $p/h = \frac{1}{2}$ implies $q/k = \frac{1}{3}$, I can deduce that $q/k = \frac{1}{3}$. This is an ordinary deduction in which the premisses and the conclusion are about probabilities: it is of no special interest. Secondly, there are deductions in which the laws of probability are premisses, but in which no analysis of the notion of probability (except that it obeys the laws) is necessary. Such are the deductions of the theorems of the calculus of probability from its fundamental axioms. For example, the Law of Great Numbers states that we may deduce from the proposition that the probability of a penny falling heads is always $\frac{1}{2}$, that the probability that the proportion of heads in n throws lies between $\frac{1}{2} - \epsilon$ and

$\frac{1}{2} + \epsilon$ (where ϵ is any small number given in advance), tends to 1 as n increases without limit. This theorem may be deduced from the axioms of probability by simple algebra by Tchebycheff's method, without involving any assumptions about the nature of probability in itself.

But the kind of "probable deduction" in which Peirce is interested is a third kind in which, while the conclusion is a proposition about probability, the explicit premisses contain no reference to probability. Such inferences require for their justification not only the formal axioms of probability, but an analysis of the concept of probability. Probability, as Peirce realises, means different things in different contexts; he defines the probability of an event (which he sometimes calls *objective probability* and contrasts with *likelihood* and *plausibility*) as the ratio of the number of events of a species to the number in a genus including that species (see 2.674). He accepts, that is, what Venn and he called "the materialistic view of the subject" and what J. M. Keynes has christened the Frequency Theory. Peirce differs from Venn in emphasising properly the fact that the probability of an event is always with reference to two classes, that it is what he calls a "relative number", the ratio of the number of PQ's to the number of Q's. But in his earlier papers (1878 and 1883) Peirce ignores the difficulties concerned in the possibility of the latter number being infinite. In 1901 he defines probability in the manner of Venn as the ratio "in the long run of experience" of these two numbers (2.785), and in 1905 he expands this into the limiting value of the ratio of the number of PQ's to the number of Q's as the number of Q's increases without limit (2.758). This is open to the objection that has been urged by, e.g., Jeffreys, that there is no unique limiting value to the ratio; and Peirce would appear to be aware of this objection in his last note on the subject (1910) when he admits that there is no "syllogistic certainty" that an unbiassed die will not turn up a six "obstinately at every single throw" (2.667). In this paper he sketches a "doctrine of chances" which seems to me the most satisfactory form for a Frequency Theory to take. The statement of an objective probability (e.g., of a die turning up six) "means that the die has a certain 'would-be'; and to say that a die has a 'would-be' is to say that it has a property, quite analogous to any *habit* that a man might have. . . . Just as it would be necessary, in order to define a man's habit, to describe how it would lead him to behave and upon what sort of occasion—albeit this statement would by no means imply that the habit *consists* in that action—so to define the die's 'would-be', it is necessary to say how it would lead the die to behave on an occasion that would bring out the full consequence of the 'would-be'; and this statement will not of itself imply that the 'would-be' of the die *consists* in such behaviour" (2.664). Thus to attribute an objective probability to an event is to state a law about events of a certain class, which theory can be neither completely confirmed nor completely refuted

by experience. In this first respect it is like every other law of nature, but the second respect is the peculiarity of laws about probabilities ("statistical" laws). Nevertheless, though such a law cannot strictly speaking be refuted by experience, for the die might turn up six always in any series of throws, yet if it did turn up six in a large number of throws, we should certainly reject the law that the probability of its turning up six was one-sixth and say (in Peirce's words) that it had taken on "an abnormal, a miraculous, habit" (2.667). This seems to me by far the best way of looking at statistical laws about objective probabilities; they are to be rejected if experience differs too widely from what the law prophesies, according to the Law of Great Numbers, as probable. The matter is more important than it was in 1910, now that physicists do not expect to be able to derive their "statistical" laws from more fundamental "deterministic" laws; so Peirce's "would-be"s seem to be some of the brute facts of the universe.¹

To return to Peirce's contention that there are deductions in which probability enters into the conclusion which are not of my first and second kinds. In his Johns Hopkins paper he gives three forms of this third sort of probable deduction, of which I will examine the last—"statistical deduction" (2.700):—

The proportion r of the M 's are P 's;

S' , S'' , S''' , etc., are a *numerous* set, taken at random from among the M 's;

Hence, *probably* and *approximately*, the proportion r of the S 's are P 's.

Peirce follows his statement of this form with an account of the relation between the "probably" and the "approximately" given by Bernoulli's form of the Law of Great Numbers: and if the conclusion is interpreted in this way, there is no doubt that it follows demonstratively from the premisses, if "at random" means that an S is equally likely to be any M . But in this case the conclusion is not in any direct sense about the proposition that the proportion r of the S 's are P 's: the conclusion is that the probability that the proportion lies between $r \pm \epsilon$ is some complicated function of ϵ . The same method of proof will prove that it is very improbable that exactly r of the S 's are P 's; so Peirce's manner of expressing the correct conclusion is very unsatisfactory.

Why he puts it the way he does is, I think, because he wishes to apply his "objective probability" to inferences as well as to events, and hence derivatively to propositions which are the conclusions of inferences. "To say that a proposition has the probability ρ means that to infer it to be true would be to follow an argument such as would carry truth with it in the ratio of frequency ρ " (2.697). On

¹ I sketched a theory similar to Peirce's, with special reference to the Quantum Theory, in the *Aristotelian Society Supplementary Volume X*. (1931), pp. 187-191.

this interpretation the "probably" attached to the conclusion of his "statistical deduction" should mean that, with a number n of inferences from the premisses to the conclusion that approximately r of the S's are P's (i.e., that the ratio of the SP's to the S's lies between $r \pm \epsilon$), the proportion of these conclusions that are true is nearly unity. But this is false: if ϵ has the value of the "probable error", the conclusion is as likely to be false as true. The true proposition is something complicated about the probability of the proportions lying within a certain range; and this, like the other correct interpretation of Peirce's "statistical deduction", is simply a case of my second kind of "probable deduction". So I do not think that a claim for a third kind can be justified.

We can now turn to "ampliative, synthetic or (loosely speaking) inductive" reasoning, of which Peirce considers two forms—induction and "hypothesis" or "abduction", which I will leave till later. The form of reasoning which Mill called "inference from particulars to particulars" and Johnson "eduction" Peirce calls "analogical inference" and treats as a compound of induction, abduction and deduction (2.733). For Peirce induction is what Broad and Mace call "statistical generalisation": he states it in the form (2.702):—

S', S'', S''', etc., form a numerous set taken at random from among the M's,
 S', S'', S''', etc., are found to be — the proportion ρ of them — P's;

Hence, *probably* and *approximately* the same proportion, ρ , of the M's are P's.

Peirce never considers separately an induction to the conclusion "All the M's are P's": this is for him a form of statistical generalisation in which $\rho = 1$ and, without the qualification of "approximately", it is never justifiable when the number of M's is infinite (2.732).

For Peirce the problem of induction is that of inferring the proportion in which a property is manifested in the world from knowledge of the proportion in which it occurs in a sample. He frequently uses the analogy of estimating the constitution of a bag containing black and white balls from the constitution of a sample of draws from the bag. He criticises effectively the two types of justification for induction usually given by logicians: Laplace's Rule of Succession, and a "uniformity of nature" major premiss. The former he demolishes admirably by showing that the "equiprobability of ratios" upon which the argument depends is untenable and inconsistent and that an "equiprobability of constitutions" will not give us anything at all (2.682 *et seq.*, 2.744 *et seq.*). The latter, which would make induction depend upon some "presupposition" like a principle of uniformity of nature or of limited variety—the method of Mill, Broad, Keynes, Jeffreys—is treated only cursorily;

but he nevertheless makes what seems to me an effective criticism of this line of approach: "To say that a theory such as these is *necessary* to explaining the validity of induction and hypothesis is to say that these modes of inference are not in themselves valid, but that their conclusions are rendered probable by being probable deductive inferences from a suppressed (and originally unknown) premiss" (2.749). In other words, if a "presupposition" were necessary, all induction would be deduction and there would be no special problem of induction.

Peirce's positive doctrine of the explanation of the validity of induction is not consistent. Three views which are closely related may be distinguished in his writings; traces of all of them are to be found in most of his papers.

(1) In his Johns Hopkins paper, where he is comparing induction with deduction, he says incidentally but explicitly that the general rule for the validity of an induction is that "the statistical deduction of which the induction is the inversion, must be valid and strong" (2.715). In the analogy of black and white balls in a bag, the induction to the proportion of black balls in the bag being probably about ρ , from knowledge that a sample has this proportion, is "valid and strong" if and only if the "statistical deduction" from knowledge that the bag has a proportion r of black balls to the proportion of black balls in the sample being probably about r is "valid and strong" (the relation between r and ρ is mysterious: see 2.720). But in most of the cases in which we are interested the bag would have to contain an infinite number of balls, which would not then permit of any "statistical deduction" and would make the criterion inapplicable. And in other cases it cannot be considered to be a sufficient condition, for if so the induction would be virtually reduced to a deduction. But what I think is correct in Peirce's contention is not that the validity of some deduction explains or guarantees the validity of an induction, but that it is a necessary condition of it. (Peirce makes this lesser claim in 2.718.) An analysis of the concept of *randomness* is difficult; but a case can be made out for saying that when the fact that the examined instances have been selected at random is demanded as a necessary condition for the validity of an induction, this fact is another way of stating Peirce's test that they should be such as would validate a statistical deduction of their properties.

(2) The justification for induction to which the greatest weight is given in the 1878 and 1883 papers is that the validity of an induction is its probability, its probability being the ratio of the number of inductions leading to true conclusions to the total number of inductions. Peirce quotes Locke as his predecessor in this doctrine, where Locke is speaking of a man who accepts a mathematical proposition on the authority of a mathematician: "In which case the foundation of his assent is the probability of the thing; the proof being such as for the most part carries truth with it" (*Essay*,

Bk. IV., ch. 15, § 1). That is, it is reasonable to believe a mathematical proposition on proper authority and it is reasonable to believe the conclusion of an induction, because both these processes—believing mathematicians on mathematics, and making inductions—are processes which “for the most part” lead to true beliefs. But for the most part of what? What is the genus whose number is to be the denominator in the fraction which is the objective probability (and hence the degree of validity) of the species of inferences in question? If it is every possible induction, or even every induction that has been or will be made, the number which carry falsehood with them must outnumber by far those that lead to the truth. (Consider, for example, all the inductions by simple enumeration—*Pooh-pooh* arguments as Peirce calls them: see 2.758 n.) The genus must be restricted to inductions obtained according to some special “precept of inference”, and every special precept that can be stated, e.g. “scientific method” is so very vague. Moreover, there are all the Frequency Theory’s difficulties about infinity. Nevertheless I think that the process of induction “being such as for the most part carries truth with it” must be an essential part of any adequate theory of the validity of the process.

(3) In the 1900’s Peirce came to see quite clearly what he had only glimpsed before, that the sort of probability involved in the question of the validity of induction is quite different from any “objective probability”. The validity does not consist in the majority of the conclusions obtained by using the inductive method being true (this is “by no means certain”: 2.781): it does consist in the fact that the inductive method is *self-corrective*. “The true guarantee of the validity of induction is that it is a method of reaching conclusions which, if it be persisted in long enough, will assuredly correct any error concerning future experience into which it may temporarily lead us” (2.769: 1905). This very interesting doctrine is presumably the one of which Peirce, in his sketch of his proposed work on logic (1902), said that when “I shall set forth the reasoning in strict accuracy of form, I defy anybody to find a flaw in it” (2.102). Unfortunately he never did this; nevertheless he wrote enough to enable us to see the trend of his argument.

Peirce was led to this view, I think, by his comparison of inductive inference with inferring the constitution of a bag from the known constitution of a sample drawn from the bag. For if the drawings from the bag are continued until all the balls in the bag have been drawn, then the proportion of black balls in the sample must approach and finally become identical with the proportion of black balls in the bag. (It would not be correct to say that the former approaches the latter as a *limit* in the technical mathematical sense, because there are only a discrete number of draws: but after n balls have been drawn of which m are black, the number of black balls in a bag containing N balls must lie between m and $m + N - n$, and the difference of these two numbers decreases by 1 with every

fresh draw until they become equal when $n = N$.) "In induction we say that the proportion ρ of the sample being P's, probably there is about the same proportion in the same lot; or, at least, if this happens not to be so, then on continuing the drawings the inference will be, not *vindicated* as in the case [of statistical deduction], but *modified* so as to become true" (2.703 : 1883). But this is only the case both if the number of balls is finite and if the balls are not replaced after being drawn: if there is no end to the number of draws there can be no assurance that ρ approaches r as the number of draws increases.

In his application of this analogy to the general problem of induction Peirce attacks this difficulty from the other end with a bold and ingenious solution. How can the universe have a character which does not manifest itself with a continued application of inductive methods? For if the universe is irregular, it must be irregular in some way, and that way can be discovered by induction. If we conceive of the universe as infinite, we must conceive of it as an infinite series governed by some law. (Here Peirce is accepting the priority of an ordinal infinity to a cardinal infinity.) "Whatever has no end can have no mode of being other than that of a law, and therefore whatever general character it may have must be describable, but the only way of describing an endless series is by stating explicitly or implicitly the law of the succession of one term upon another. But every such term has a finite ordinal place from the beginning and therefore, if it presents any regularity for all finite successions from the beginning, it presents the same regularity throughout" (5.170). But, granted that an infinite series must be governed by some law, it by no means follows that the law can be discovered, or even approximated to, by any finite series of observations. A mathematician can easily construct a series whose rule will not be discovered (except by a lucky guess) from inspection of any number of its terms; and there is no reason to suppose the Creator less ingenious. Moreover, even if an infinite series of events had a regularity that could be easily discovered, there is little reason to suppose that examining events of that series in a different order would enable us to discover the regularity of the series in the original order. Suppose that there are alive at this moment 1000 times as many white swans as black swans. The correct proportion would only be discovered by a set of people who made one count in Australia to 1000 counts elsewhere. Peirce may very well be right in insisting that the only way in which we can conceive of infinity is as an ordered series, but he must admit that it can have different orders. Since this is the case, the only way to discover the order of the universe would be to pursue the inductive process not only infinitely but also exhaustively. We cannot do the first of these, let alone the second.

The element of truth in Peirce's argument is that irregularity in the universe is in itself no bar to the success of induction. "Utter irregularity is not surpassed in regularity by any other relation of

parts to whole, and is thus readily discovered by induction to exist where it does exist" (2.769): this has been shown recently by the success of the Uncertainty Principle in physics. But it is quite a different thing to say that, if the world is suitably irregular, induction can lead to the truth, than to say that it must do so, whatever the constitution of the world.

I have assumed that the "wonderful self-correcting nature of ampliative inference" (2.749) with which Peirce is concerned consists in the substitution of a more correct value of ρ in the inductive conclusion that a proportion ρ of the S's are P's, as described in his 1883 paper. For later, when Peirce praises this "self-correcting nature" so much, he never explains precisely what it is: and he may be thinking of another sense in which induction may be said to correct itself—that in which we substitute the hypothesis that a proportion ρ of the SQ's are P's for the hypothesis that a proportion ρ of the S's are P's when this is refuted or rendered very unplausible by experience. This sense seems to me quite as important a feature of inductive procedure as the other: Peirce's remarks that "for sound reason . . . it is necessary to take account of the manner in which the [examined] S's were brought to the inquirer's attention" and on the importance of the "general conditions" accompanying the examined S's (2.763) are more relevant to this second form of self-correction.

What seems to be the most important point in Peirce's teaching, which is common to his second and third theory, is that a particular inductive inference is not to be considered as valid in itself, but valid as a member of a class of inferences whose defining property is that they are obtained by "inductive method". That is, the "justification of induction" is essentially the justification of the inductive method. Peirce's second theory justifies the inductive method by saying that it leads on the whole to the truth, his third theory by saying that, if persisted in, it tends to correct itself. It seems to me that a more satisfactory theory of induction than either of Peirce's separately can be produced by combining their strong points, in some such way as this: The validity of an inference made by inductive method (and inductive method must be limited to some sort of scientific method so that not all inductions by simple enumeration are included) consists in the fact that most of the "developed" inferences lead to true conclusions, a "developed" inference being one obtained after the failure of many inferences in the same field have corrected the conclusion (either by changing the ρ or by limiting the S to SQ). What is meant by "most of the inferences lead to true conclusions" is the same sort of thing as what is meant by "most dice turn up six between 16 per cent. and 17 per cent. of the throws" (see my account of Peirce's "would-be's"): the class referred to as "the developed inferences made by inductive method" is not the class of inferences that are in fact made, nor the class that could conceivably be made, but the class of those that could be

made by creatures with intellectual faculties rather like ourselves. (Cf. Peirce's "social principle" in logic: 2.654.)

Such a theory as this will yield, I think, a satisfactory, though vague, criterion of the validity of induction. But, a critic will say, it cannot be an explanation or a definition of that validity, since our knowledge that most inductive inferences of this sort lead to the truth is itself derived from induction. It is curious that this difficulty, which has been prominent in recent discussions of induction, is nowhere discussed by Peirce. It is true that he frequently talks of inference as essentially a habit; but he never says explicitly, as Ramsey says, that induction "is one of the ultimate sources of knowledge just as memory is", so that "if any one has not got the habit of induction, we cannot prove to him that he is wrong" (*Foundations of Mathematics*, p. 197). Thus that part of Ramsey's essay which, he says, "is almost entirely based on the writings of C. S. Peirce" (*loc. cit.*, p. 194 n.), is a continuation and not merely a re-statement of Peirce's line of thought. It seems to me that the Peirce-Ramsey method of approach, incomplete though it is at present, is the only one that properly appreciates the peculiarities in the problem of the justification of induction.

Something must be said about the other form of ampliative reasoning which Peirce calls at different times "hypothesis", "abduction", "presumption", "retroduction", and to which he attaches great importance. In his papers of 1878 and 1883 he treats deduction, induction and abduction as three types of inference which are equally fundamental, and he produces a complicated (and, I think, misleading) doctrine of their mutual relations treated with reference to syllogisms (2.508, 2.623, 2.716). Abduction is here an argument of the form (2.623):—

Every M is P,

Every S is P;

Hence, every S is M;

the class of S's being a class given by enumeration—[a, b, c, d]—and P being a conjunction of properties—P'P''P''''. Whereas induction infers that all the members of a class have a property of some of the members of the class, abduction infers that a property which formally implies (in Russell's sense) a set of properties applies to the things having this set of properties. An abduction is, as Peirce notes,¹ reducible to an induction applied to properties. Never-

¹ Peirce does not set out the reduction of an abduction to an induction with sufficient precision (2.632): the explicit reduction is:—

Every member of the class of properties [P', P'', P'''] is a property of everything of which M is a property,

Every member of the class of properties [P', P'', P'''] is a property of every member of [a, b, c, d];

Hence, inductively, every property of everything of which M is a property is a property of every member of [a, b, c, d];

Hence, deductively, M is a property of every member of [a, b, c, d].

theless he thinks it important to treat it separately, partly because "characters have to be weighed rather than counted" (2.706) so that the multiplication of instances works differently in the two cases, but chiefly because "the essence of an induction is that it infers from one set of facts another set of similar facts, whereas hypothesis [abduction] infers from facts of one kind to facts of another" (2.642). However, in the "Minute Logic" of 1902 Peirce admits that his early papers attached too much importance to syllogistic forms and "confused two different kinds of reasoning" in the conception of Abduction (2.102). Consequently in his later papers "abduction" and its synonyms are used, not for a process of inference parallel to induction, but for the "probational adoption of a hypothesis" (2.96) which has then to be confirmed or refuted by induction (see 2.776, 2.755). This, of course, is the ordinary account of scientific procedure given by every logician since Jevons: Peirce only differs from the orthodox account in classifying the thinking of the hypothesis as itself a form of ampliative reasoning, and this difference is, I think, merely a verbal one, namely, that of whether "an act of *insight*, although of extremely fallible insight" (5.181), is or is not called reasoning. (In 2.643 he calls abduction an emotion!)

In various places throughout Volume II. Peirce gives practical rules for abducting hypotheses, some of which (*e.g.* lack of bias) are excellent, but one of which—that the hypothesis must be specified "in advance of, or at least quite independently of, any examination of the facts" (2.789: see 2.735 *et seq.*)—seems to me only important if one takes the lowest view of the credulity of scientists. He recognises, however, that the main philosophical questions about the use of hypotheses are concerned with what hypotheses are possible, which is part of the general question of the significance of propositions, to which "pragmaticism" has a definite answer. "If you carefully consider the question of pragmatism you will see that it is nothing else than the question of the logic of abduction" (5.196). So his final treatment of abduction is to be found in the Harvard Lectures on Pragmatism of 1903; and I leave them for discussion to the reviewer of Volume V.

Peirce says of Kant that he "possessed in a high degree all seven of the mental qualifications of a philosopher:

1. The ability to discern what is before one's consciousness.
2. Inventive originality.
3. Generalizing power.
4. Subtlety.
5. Critical severity and sense of fact.
6. Systematic procedure.
7. Energy, diligence, persistency, and exclusive devotion to philosophy" (1.522).

How many of these qualifications did Peirce himself possess? 2, 3, 4 certainly (indeed he had 3 over-developed); 7, on the internal evidence, I think he had, and 6 he more often attempted than achieved. His deficiencies were in 1 and 5. His sense of fact, though sometimes extremely hard-headed, deserted him when his metaphysical fancy led him on; and his critical severity was blinded whenever he met a triad. Reared on the *Critique of Pure Reason* and the J. S. Mill tradition transmitted through Chauncey Wright (1.4), he had the defects of his masters. But he also had many of their virtues. He combined a commonsensical outlook with creative imagination to a higher degree than did Mill, and his professional knowledge of pure and applied physics kept his logic closer to actual scientific thinking. And Peirce had one qualification which, though perhaps not necessary to a pure philosopher thinking by himself, is very important to one who wishes to influence the thought of others—that of being a sufficiently good writer not to turn his gold into dross. Peirce's writing is of very varied texture—sometimes abstract, sometimes concrete or personal; but it is nearly always readable and occasionally remarkably felicitous. He frequently digresses, but his digressions are so full of obscure and interesting information that a curious reader will find himself forgiving him for them. (Since reading 2.252 I shall never mis-spell "premisses".) He is not a writer to read at a stretch, but one to dip into: the fluidity of his thought makes it more stimulating. It may well be that philosophy will derive more from this admirable edition of Peirce's "Collected Papers" than it would have done had he been able, as he wished, to freeze his philosophic imaginings into an architectonic *magnum opus*.

R. B. BRAITHWAITE.

Beauty and Other Forms of Value. By S. ALEXANDER. London: Macmillan & Co. Ltd., 1933. Pp. 305. 10s. 6d.

In this book Prof. Alexander considers many interesting problems, suggests some original hypotheses, makes a good use of examples to illustrate his points, and shows a strong and understanding interest in works of art. Indeed his comments on the works of art he likes form a very attractive feature of his book. Though his main interest is in aesthetic problems, he also wishes to show that they form the best starting-point for solving problems about moral values and truth. In this review, however, I shall only discuss his aesthetic views, and propose to consider in the first place how he makes use of the "constructive impulse become contemplative". He takes this impulse to play a very important part in the answers to a great many aesthetic problems, and we want to know both what it is, and in what way it enters into these answers. As he himself has not

clearly distinguished these problems from one another we must do it for ourselves.

We can begin with some purely psychological matters. Prof. Alexander asks what leads to the production of works of art, and answers: the constructive impulse become contemplative. This has developed he tells us out of the constructive impulse as we find it in animals, for example in birds when they build their nests in order to protect their young. It becomes contemplative when it becomes an impulse to handle materials for their own sake, and not merely for some practical end—this is what he means by calling it “contemplative” (p. 19). Then he asks: “How is this transition effected?” (p. 19) and answers: “the artist mixes himself with his materials” (p. 19); “he imputes to his materials characters which in practical experience they do not possess” (p. 20)—and does this through the form which he gives to his materials. For example he takes a block of marble, and shapes it into a statue which appears “alive and divine and playful” (p. 36). Or (p. 42) he selects a number of tones, so that they have movement, melody and rhythm—from which it can be seen that the expression “imputation of characters” is used in a very wide sense. We are not told whether it is given as a definition of “he handles his materials for their own sake”, but Prof. Alexander uses all the expressions I have given as equivalent to each other.

I find it difficult to say anything about this point, for to consider it properly one would need to have made an empirical investigation into the motives of artists. Looking at the matter without knowledge obtained through such an investigation, we should hardly want to deny that artists do usually create because they want to handle materials for their own sakes and to impute characters in the senses considered. On the other hand, it is clear that they also create for other reasons, *e.g.* in order to solve the technical problem of how to represent the effects of light. And I think the impulse to construct for *practical* ends is more operative than Prof. Alexander's words suggest. Consider, for example, that the architect's main aim is often to construct buildings which will best serve their uses—and inspired by this aim he may produce very beautiful buildings.

Let us now consider the constructive impulse become contemplative in relation to contemplation—that is, for example, in relation to the spectator's activity. (I shall often speak about the spectator when what is being said refers also to the reader and listener. I can think of no word that applies to all and am biased in favour of the spectator because most of the remarks I shall make are suggested through experience of visual art.)

I find it very difficult to find out just what Prof. Alexander wants to say about this matter—and perhaps his main point has just escaped me. On page 14 after remarking that we contemplate objects for their own sakes he says: “We have to ask what if any are the motives or impulses which lead to this intrinsic contemplation of objects”.

I found this remark rather startling as it seems natural to say that often my motive for contemplating is that I want to contemplate. Indeed if I am not contemplating an object because I want to contemplate it, I am not contemplating it for its own sake. As we read further we see that Prof. Alexander wants to find "the impulsive basis of æsthetic apprehension" and finds it in the constructive impulse become contemplative. But in what sense is it a basis?

The trouble is that he has not distinguished between different possibilities, but his language none the less suggests them. And when we are given a reason it is difficult to see what it is a reason for. Is he saying that the *cause* of my wanting to contemplate is my wanting to construct contemplatively? The only reason for this we can find in what he says is that contemplating and constructing are very similar, which is hardly a good reason. Is he saying that my only *reason* for contemplating is wanting to satisfy my constructive impulse become contemplative? If so he is saying that wanting to contemplate is never a reason for contemplation—which I am sure is not the case. Is he saying that contemplating is itself a form of constructing contemplatively? Or is he saying that the pleasure we get in contemplating is the pleasure of satisfying the constructive impulse? (This is certainly *one* thing he means to say, and we shall consider it later.)

But though I can say no more as to what Prof. Alexander means to say about the impulsive basis of contemplation, I shall say something at this point about the nature of contemplation. Prof. Alexander always emphasises its constructive character, yet there is a tremendous difference between the spectator's and artist's construction. This indeed is pointing to the obvious, but the reason for my insisting on the difference is that Professor Alexander is trying as hard as he can to minimise it.

Let us briefly consider in what ways the spectator constructs. Prof. Alexander points out that the spectator also imputes characters to materials which do not possess them. For example, he sees a piece of marble and it appears to him to be "alive and divine and playful", but in fact it is not; or he sees a pattern which he has to construct in the sense that it only gradually becomes clear and he only gradually distinguishes its interrelations. Prof. Alexander believes indeed that however clearly he sees these interrelations and however familiar with them he may be, yet he is still constructing (p. 47 and p. 185)—which I take also to be the *Gestalt* view of spatial perception. But what an attenuated form of construction!

Now it is clear that the spectator in constructing cannot be constructing contemplatively in the sense that Prof. Alexander has defined and which includes the *handling* of materials. And the spectator cannot be imputing characters in the same sense as the artist imputes them, for to talk about the artist's imputation of characters is to say that he forms his material by handling it in such a way that certain spectators who perceive it will impute certain char-

acters to it (in another sense—a sense which does *not* include handling anything).

Prof. Alexander's language usually obscures these differences and he never draws them explicitly. Thus it is not surprising that he agrees with Croce's view on the subject (though usually he shows a healthy disagreement with Croce's views). He says (p. 29): "As Mr. Croce makes clear, appreciation of beautiful art is to repeat the creation of it, so far as the spectator can" and (p. 30): "There is therefore no difference in kind between æsthetic appreciation and æsthetic creation. The one is the pale shadow of the other, as passively at the suggestion of the accomplished work the spectator recovers the creator's mind".

I have already given reasons for objecting to the phrase "no difference of kind". I now want heartily to disagree with what Mr. Croce is asserted to have made clear. It is most important to distinguish between the artist's mind when he creates and when having created his work he contemplates it. In creation (as Prof. Alexander has pointed out elsewhere) he goes through a process of trial and error (at least in a great many cases). If a writer he makes rough notes, if a painter he makes rough sketches. It would be most inimical to the spectator's desire for æsthetic contemplation were he to recapture such experiences. He leaves that to those who are interested in the psychology of creation. If he is to recapture the artist's state of mind at all it is the state where having finished creating he becomes himself a spectator. And in this case the phrase "pale shadow of creation" is quite inept, for the spectator's experience in contemplating may be quite as vivid as the artist's.

And now we shall turn to problems of quite a different kind—though still problems connected with the impulse to construct contemplatively. In chap. iii. Prof. Alexander considers the characters in virtue of which we call an object beautiful, and says: "We can use the nature of the impulse as a key to lay open the character of the object which corresponds to it and gratifies it, instead of doing as writers in æsthetics are apt to do, without convincing success, who endeavour to analyse the nature of the object itself". This passage involves Prof. Alexander's definition of beauty, but we can disagree with the method of discovery he suggests without having considered this definition. And my reason for disagreeing with it is compatible (I think) with his view that in contemplating we impute characters to the physical object we perceive. To see this we must briefly consider the relation of 'fusion' which in Prof. Alexander's view relates the physical object to its imputed characters. We can see what he means if we again consider the marble which looks "alive and divine and playful". We see these characters *in the marble*, it does not merely suggest them to us.

My reason for disagreeing with Prof. Alexander's view that to consider the impulse is better than to analyse the object is that even if contemplation is constructive to the extent which he believes,

yet the physical object and its imputed characters which I perceive, are objective in an important sense—they are what I perceive, part of my visual field, and objective in this sense however dependent they may be on construction. So the best way for me to discover the characters of the beautiful is to study the work of art which I perceive. And in doing so I shall see that my construction is *not* included in my visual field—nor (unless I am looking at a ballet or a play) is any kind of construction included in it. Consider such a statement as this made by Prof. Roger Fry: "Behind, a tree divides the composition in half with the rigid vertical of its trunk, above which its foliage forms an almost symmetrical pyramid, which is completed and amplified by the group of houses behind".¹ Facts of this kind are among the reasons for judging a picture to be beautiful and they are *not* facts about construction (if that term stands for any kind of activity or for the means which a painter uses to obtain certain visual appearances).

I am not denying, however, that knowledge of what the artist wants to create *sometimes* helps us to discover why an object is beautiful—in which case we may say we are using his impulse as a key. Nor am I denying that æsthetic judgments are often about construction. Very important judgments are made about construction—especially by artists since they are interested in problems of how to get certain effects. But they simply constitute one class of æsthetic judgments.

We have now come to the last of the questions to be considered in relation to the constructive impulse become contemplative—the definition of beauty. Prof. Alexander's definition in its most general form is this: to say that an object is beautiful is to say that it gives pleasure after a certain manner to a "standard mind" (*i.e.*, qualified persons). To say that it gives pleasure after a certain manner is to say that it "satisfies . . . the constructive impulse diverted from practice and become contemplative" (p. 35); or as he also puts it (p. 50), "produces in the mind a special pleasure of satisfying a certain impulse in the mind to produce such external objects" (*i.e.*, works of art). Compare also: The pleasure we get in experiencing beauty "has its peculiar feel because it is the pleasure of satisfaction of the æsthetic impulse, and attaches as the pleasure of sugar attaches to an object of a certain kind" (184), and "Satisfactoriness is attributed to the object . . . just as food is naturally described as pleasant because it satisfies the impulse to eat".

Here we are in the midst of psychological problems, and I must try to do what I can about them. And first I want to ask which impulse is satisfied—the impulse to handle materials or to contemplate? (*i.e.*, construct in this sense). The quotation from p. 50 answers that it is the first—but why then does Prof. Alexander say that our only pleasure in contemplating a beautiful object is the pleasure of constructive exercise? (p. 184).

¹ Cézanne, p 61.

In the second place I want to distinguish, as I take it Prof. Alexander would not, between saying that sugar is pleasant, i.e. is the object of pleasure, and saying that sugar satisfies hunger. And similarly I would distinguish between pleasure in a work of art (in its spatial interrelations, etc.) and its giving satisfaction to the constructive impulse—or any other impulse, for a work of art clearly satisfies many. Satisfying an impulse may be the *cause* of our pleasure, but when we call a work of art beautiful we are often only speaking about that work of art itself (its spatial interrelations, etc.)—i.e. the *object* of pleasure. As for the “peculiar feel” of the pleasure to which Prof. Alexander alludes, I should explain that in terms of the *object*, our pleasure is in “particular features of the formed material”, and not in something else—and this is what we mean by its “peculiar feel”.

I must confess that I have found myself disagreeing with Prof. Alexander on a good many views about the constructive impulse become contemplative. But I want to add that he is doing good service by emphasising the importance of construction. His views are well founded in this sense that they have developed out of his belief in this importance. I shall now turn to some other problems, and first to the distinction between “poetry and prose in the arts” (ch. vi.).

Here we have a very interesting distinction, which I have never met before, and in addition Prof. Alexander illustrates his points by discriminating remarks about individual works of art. His aim is to indicate a distinction between works of æsthetic merit (others are excluded) which applies in the same sense to all the arts, but which has only been named in literature. Accordingly he first considers the distinction in literature, and we shall see that it has in fact not been named because it only corresponds approximately to the ways in which “poetry” and “prose” are actually used. It is an important distinction none the less. Prof. Alexander first tells us that he is not speaking of the formal distinction between poetry and prose, but of that on which it depends. Is he, we may ask, speaking of the way “poetic” and “prosaic” are used when we speak of poetic prose and prosaic verse? This is much nearer the mark, but doesn’t exactly hit it. His distinction corresponds to a large extent to the distinctions made in ordinary language, but does not exactly correspond with either.

His case rests largely on an admirable choice of illustrations. He quotes a number of passages in pairs, one member of each pair being prose, the other poetry, and both members having the same subject-matter. He then bases his descriptions of the difference on these illustrations, and it is natural that the former should appear rather empty without the latter. He says that prose analyses, describes, reconstructs and gains its life from the subject outside it, while a poem “is like an animal or plant”, it lives in itself and creates instead of describing its subject—thus for example the passage in

King John about grief (Act 3, Sc. 4) "is not a picture of grief but grief enacted" (97). Poetry in this sense is usually poetry in the ordinary sense because rhythm and metre are "unifying instruments". Prof. Alexander admits that many works of art are neither poetry nor prose, but somewhere along the line that leads from one to the other. I believe he is drawing an important distinction, and we can easily find examples of poems and prose works in his sense. Compare, for example, Stendhal's *Le Rouge et le Noir* with Tolstoy's *War and Peace*. Stendhal analyses his characters and Tolstoy creates them—I don't think, however, that in ordinary language we should call *War and Peace* poetic prose.

I can also see this distinction in painting. We should agree with Prof. Alexander that Botticelli's *Venus* is poetic, and Daumier's *Gare St. Lazare* is prosaic, and we can see two distinctions between these paintings depending on the relation of form to representation—which is what Prof. Alexander takes to be the determining feature. The *Gare St. Lazare* is 'realistic' while in the *Venus* we find that "heightening of the emotional pitch which makes poetry".¹ But more important still from Prof. Alexander's point of view, the former is descriptive and the latter is creative, it has 'inner life', to use Prof. Roger Fry's phrase. But in this case too Prof. Alexander's distinction does not correspond to our use of "poetic and prosaic painting" in ordinary language. For many a painting has 'inner life' yet would not be called poetic—as for example Van Gogh's painting of boots. In painting as in literature the ordinary distinction applies within a narrower range.

Prof. Alexander believes the distinction to be most salient in architecture (apart that is from literature) and yet it seems to me that he is here trying to describe a different distinction, and that it is not a real one. In the first place he says that the subject-matter of architecture is its utility—but utility is not equivalent to representation. In the second place he says that a building is poetic when it has a life of its own and doesn't suggest its uses. It is prosaic when although "its unity and organic characters are maintained" (105) yet it "bears upon its face the purpose of its existence". But any beautiful building can be considered purely as a spatial organisation—the Royal Crescent at Bath (prose) as much as Salisbury Cathedral (poetry). On the other hand, we can also consider the spatial organisation of any beautiful building in relation to its uses, and I can't see that Salisbury Cathedral is differently related to its use than the Royal Crescent. We do use the terms "poetic" and "prosaic" for buildings as well as for painting, and should say that Salisbury Cathedral is poetic and the Royal Crescent prosaic—but not for the reasons which Prof. Alexander gives. Here again the ordinary distinction does not correspond with his.

I do not think Prof. Alexander has successfully defended the

¹ *British Painting*, Roger Fry, p. 61.

"Objectivity of Beauty" (ch. x.). He has defended himself very well against certain objections but does not give a satisfactory account of the "standard mind". A thing is beautiful only if it satisfies the constructive impulse of the "standard mind",—a phrase which certainly needs translation in order to be significant, and Prof. Alexander translates it into "qualified persons". And then we expect some account of what is meant by "a qualified person". I think an account might be given, but Prof. Alexander does not give it. He says: "The standard æsthetic sentiment is that of qualified persons, and those persons are qualified who possess the standard æsthetic sentiment" (176). This does not merely appear to be circular, as Prof. Alexander admits, but *is* circular. And just above, the word 'beauty' occurs in the statement that defines it. He agrees that in his view there would be no "fixed standard", and I agree with him in not regarding this as an objection.

Prof. Alexander has some interesting things to say about the "creative process in the artist's mind" (ch. iv.). He has evidently given careful consideration to this matter. He contests Croce's view that "the artistic experience . . . is purely mental" and "that the actual physical embodiment of the experience . . . merely serves the purpose of communication" (57). In the first place he has frequently pointed out elsewhere that the judgment, "this is beautiful", applies to a physical object. Here he points out that in most cases the artist does not start with an image, which he then copies into some material, but that he shapes and reshapes his material in order to mould the physical object into a certain form. And speaking of poetry he says: "the poem is not the translation of the poet's state of mind, for he does not know till he has said it either what he wants to say or how he shall say it" (59). And this applies to every art—that is, it applies very frequently. In this chapter and the next (on Form and Subject-matter) he makes very apt remarks on the relation of subject-matter to form.

HELEN KNIGHT.

La Pensée et le Mouvant : Essais et Conférences. By HENRI BERGSON.
Paris, Alcan, 1934. Pp. 323. 25 fr.

IN this volume M. Bergson collects seven essays or lectures written between 1903 and 1923, and introduces them with two chapters of introduction (composed specially for the purpose) which he dates January, 1922. In certain footnotes and in other ways, however—the most elaborate of these footnotes being a retrospective account of his intentions in *Durée et simultanéité* (1922)—he indicates briefly his alert interest in the science and philosophy of the next decade; and the essay on "Possibility and Actuality", although stated to

be a development of M. Bergson's address to the Oxford Conference of 1920, made its first and only printed appearance in the Swedish language (*Nordisk Tidskrift*, Nov., 1930). The six remaining essays are: "Philosophical Intuition" (given to the Bologna Congress in 1911), "The Perception of Change" (lectures given at Oxford in 1911), "An Introduction to Metaphysics" (from the *Revue de Métaphysique et de Morale*, 1903, and very well known to English readers from the late T. E. Hulme's translation of it published in 1913), discourses on Claude Bernard (1913, on the occasion of the centenary of that great scientist's birth) and on Ravaisson (published 1904 and given to the Académie des Sciences morales et politiques on M. Bergson's entrance to that body as Ravaisson's successor) together with the author's introduction to the French translation of William James's *Pragmatism* (1911). The two introductory chapters deal principally with the problem of philosophical method, and really are an introduction to the rest of the book; for all the essays deal with that theme.

Since M. Bergson is universally known to be one of the few great masters of philosophy in the present age, it is as superfluous as it would be impertinent to commend these essays. In style, lucidity and depth, they are of the author's skilful best; and that is all that need be said in this vein, unless it be that the problem of philosophical method has always been central in M. Bergson's thought, and consequently that the bringing of these essays together, with a full introduction emphasising their indisputable unity, is a very great service to all who are attracted by M. Bergson's ideas, that is to say, to all contemporary philosophers.

This being understood, I shall attempt to make certain observations concerning the problem with which M. Bergson was confronted and the nature of the method by which he hoped to solve it.

As M. Bergson notes in his essay on Ravaisson (p. 303), the later nineteenth century might be called the biological era of science. It was natural, therefore, for M. Bergson in his youth to be profoundly influenced by the philosophy of evolution, particularly by Spencer's form of it (pp. 8, 118); and he soon became convinced that a philosophy of development in which temporal process was not itself philosophically developed was empty and even frivolous. He may also have been fortified in this conviction by Claude Bernard's fidelity to the facts of biological individuality and also to an "idée organisatrice et créatrice" (without prejudice against physics and chemistry) in that domain (pp. 261, 304); and in any case he is now prepared to say that Bernard's *Introduction à la Médecine Expérimentale* is for us what Descartes's *Discours* was to the seventeenth and eighteenth centuries.

Accordingly the essential problem was to evolve a philosophy adequate to a biologically-minded age; for the two types of philosophy that predominated during M. Bergson's philosophical novitiate—I mean, positivism and Kantianism—were pre-evolutionary and

un-biological. Of the former, M. Bergson says relatively little in these pages, although he remarks that Comte had no flair for biology (p. 303), and shows in various places (*e.g.*, p. 220) that an associationistic empiricism, like Mill's, had no proper understanding of the immediate or, in William James's phrase, of the *pure* experience of the *moi psychologique*. In any case M. Bergson became convinced that positivism and agnosticism must be mistaken, since man's experience could and did auscultate something absolute. On the other hand, it is evident from these essays that Kantianism was very much in M. Bergson's thoughts. Indeed, one might say that he set himself to devise a remodelled and inverted Kantianism, suitable for a philosophy with which the name of Bernard counted for more than the name of Descartes.

Kant, he says, proved incontestably that the understanding could work only with the material supplied by intuition. That was his great discovery (p. 176); but he misconceived the authentic character of intuition, both when he employed it and when he despaired of it. When he employed it, he treated "representations" as a chaos of desiccated fixities, just as the associationists did; and he attempted vainly to make the chaos orderly by a sterile apparatus of forms and schemata,—an empty space encompassing (but not really *within*) matter-of-fact; a void immobile time; a vast armament of categories devised wholly in the spirit of the Platonic and mathematical eras in philosophy although, as the pragmatists later saw (p. 278), man's spirit and all its genuine categories must be *growing* and *adventurous*.

A remodelled Kantianism, therefore, must accept the evolution both of mind and of the universe, that single, great reality. Accepting time as the "form of the inner sense" it must treat time for what it is, *viz.*, a spiritual life and growth, and not like Kant as something that must be intuited as a line, that is to say as a spatial fixture in one dimension. It must eschew a lifeless "permanent" or "substance" (*e.g.*, p. 89) and regard space, not as the order of the un-moving, but, like "rest", as an aspect or complexity of change (*e.g.*, p. 110). Further, and even more importantly, the loyal and enthusiastic acceptance of Kant's grand discovery concerning "intuition" should lead not (as with Kant when he despaired) to the reluctantly abandoned dream of a supersensible intuition in metaphysics, but to an inverted Kantianism in which "intuition" is explored within its natural home in life, spirit and time. Intuition is a wholly natural fact, self-vouching and self-explanatory if only we accept it for what it is. Kant, quite correctly, showed that the typical traditional intellectualisation of sensible representations "lived and died of antinomies" (p. 252) when the attempt was made to stretch it metaphysically beyond possible experience. Where he went wrong was in assuming that *if* there were metaphysics, there would have to be a supersensible intuition, the truth being that the only and sufficient intuition that metaphysics needs is within us

from the outset and always ; for it is just our natural selves and our own inner life.

According to M. Bergson *esse est experiri*. "In general," he says (p. 46 n.), "we mean by reality what is given in some experience, or might be so given. That is real which is 'constaté ou constatable'" (cf., p. 61). Experience, then, yields absolute reality ; but it is not confined to sense-perception ; and therefore "science", the elaboration of sense-perception, is not the only way of knowing. There is also "intuition", that is (in the main) self-acquaintance. And such "intuition" can also be elaborated. If one asks how far intuition can go, the answer is that intuition alone can tell (p. 61). But the intuitional is, *par excellence*, the metaphysical method ; and metaphysics is, primarily, just what intuition reveals.

M. Bergson would like to be on good terms with science. Sense-perception, he says, is genuine experience. Therefore it is absolute (however limited and superficial). Therefore scientists are unnecessarily modest if they profess mere positivism with its agnostic corollary. M. Bergson's friendliness towards scientific method, however, is combined with very severe criticisms of Platonising, Cartesian, mathematical science, and yet with an odd defence (it would seem) of these very scientific errors. Some mention should therefore be made of these matters before proceeding to consider the constructive, metaphysical side of M. Bergson's method.

M. Bergson is prepared to take a realistic view of perception. "We perceive objects," he says (p. 95), "in themselves, not in ourselves". He holds, however, that the inevitable limitations of perception preclude it from sufficing even for scientific purposes. It is incurably superficial (p. 27), a mere sketch (p. 217), a divider where Nature has not divided (pp. 172, 207), and, most important of all, incapable of seizing the reality of movement and passage. Perception freezes movement into immobility, and the attempt to reconstruct change with immovable perceptual selections is necessarily abortive.

Here, surely, there is room for dubiety. Keeping to the main point, have we not the right to ask *why* it must be assumed that a perceptual selection within a movement necessarily freezes or immobilises that movement ? According to M. Bergson, there would be no surmounting Zeno's paradoxes regarding Achilles and the tortoise, if we could not transcend our perceptual limitations and enter into the experience of Achilles himself who, aware that he takes unitary strides, has no difficulty in understanding how he outstrips the tortoise. But why should not the spectators perceive Achilles winning the race ? And if they remember his victory, can it justly be concluded that because the events they remember are fixed and unalterable there is no recollection of *movement* ? Again, does not time itself involve the kind of limitation that M. Bergson deplures ? A growing thing, experiencing its growth, does not thereby *stop* its growth ; but it cannot experience more than the growth that has

occurred up to and including the moment when it does experience that growth.

There are difficulties, therefore, regarding M. Bergson's account of perception, and, I think, there are even greater difficulties regarding his attitude towards the traditional attempts of "science" to "prolong" perception by the employment of conceptual fixities. Let us set aside M. Bergson's horror of shadows (p. 138) and of mere symbols (p. 206) and of *homo loquax* (p. 106) as being parts of the usual pleasantries natural to the occasion, and come to more substantial questions. Is it seriously to be inferred that because concepts are wholly definite, or even in some sense eternal, there can be no adequate concepts of fluidity or of change? And what could generalisation be if there were no generality in Nature? M. Bergson himself argues that order in nature is prior to disorder just as existence is prior to nothingness (p. 125), and he does occasionally admit that there really is objective generality (p. 69). For the most part, however, he tries to persuade us that generality is really *in us*, being a name for our fixed habits. If so, how can he also hold (as he does) that these habits are supremely useful to us, unless our habitual expectations are fulfilled, that is to say, unless Nature really has the fixity in question? It is surely neither useful nor sensible to treat fluidity as if it were fixed. Mr. F. L. Wright, the American architect, built the only large new building in Tokio that withstood the earthquake because he built it to float on the mud. The others, not he, were unpractical.

Indeed, M. Bergson's confidence in the utility of perception and of conception, despite the inadequacy or even the make-believe that he professes to discern in them, is very strange indeed. Perception, he explains, has the usefulness that blinkers have to a horse (p. 172); and we may admit that carriage-horses are the better for their blinkers, provided that the coachman does not have blinkers. It is not the case, however, that *all* horses should have blinkers, or that Nature made a mistake by omitting to supply them with natural blinkers. Again, M. Bergson tells us that socialisation is fixation (*e.g.*, p. 102)—as if society were a better hand at immobilising the mobile than the late King Canute. Sometimes he speaks as if there really *were* "matter", that is to say mere repetition of self-same identities; an unchanging substratum or support of qualities (and so forth); and as if conceptual intelligence were at home in this department of the universe and in no other. Yet at other times we learn that it is not so, although it is extremely useful for us to behave as if it were so. In that case, I submit, Nature might be trusted to unmask the pretence. True, M. Bergson tells us that we should look for immobility within movement, not outside or alongside it; but we have to complain, I think, that many of his most vivid illustrations have little or no meaning unless there is "matter" in the old-fashioned sense, which he disputes.

Let us turn, however, to "intuition".

As it seems to me there are several perplexities attendant on M. Bergson's approach to his central theme.

In the first place his account of the relation between the self and the not-self (or the half-self) seems to be hesitating. When he introspects, he says (p. 207), he finds a superficial congelation of detached morsels of perception, memory and motor habits; and, beneath them all, *durée* itself. This language is obviously metaphorical, and the metaphors do not seem to be very helpful. It is not clear whether these detached or semi-detached morsels really exist or don't, or again, if they *are* an external crust, whether they are part of the self (as a lobster's shell, I should suppose, is part of a lobster) or are not a part of it (as barnacles are not part of a ship). The explanation (p. 244 n.) that they are secondary and derivative, elements and not parts, is stated more soberly, but is still rather vague. And M. Bergson's readers have to make what they can of the statements (a) that intuition occurs, by surplusage, when a mind is attending to its proper object, matter (p. 98); and (b) that we *do* experience the material and vital forces in the world outside us (p. 157).

In the second place, M. Bergson seems to make certain assumptions about the essential nature of knowledge which he might find some difficulty in defending. Holding that it is much more difficult to be well acquainted with ourselves than with the external world (a doctrine that advocates of *Nosce teipsum* have always strenuously resisted), he nevertheless expresses surprise at the circumstance (p. 50), thus indicating that if we *are* such and such, there is some antecedent probability that we ought to be able to know it, and know it better than anything other than ourselves. Similarly he holds that if we have done anything, we ought, self-evidently, to be able to know what we have done (p. 75). Any other view, he says, suggests an impossible aloofness about knowing, as if the knower had been "put in the corner" (p. 157).

But why should all this be plausible? Certainly we are what we are, and have done what we have done; but a stone is what it is, and has fallen when it has fallen. To be and to perform, therefore, is not necessarily to know. Is the contention, then, that *if* an entity is able to know, it *ought* primarily to know itself, and to know other things only derivatively? This may be so, but why should it be so? What is absurd in the view that knowledge presupposes a certain aloofness and "psychic distance" even when it is self-knowledge? Indeed, what is *absurd* in the view (whether or not it is true) that we *cannot* attend to ourselves, and can only attend to something other than ourselves?

In the third place, if intuition be an auscultation (p. 202) or a savouring (p. 107) of *ourselves*—a matter of knowing *because* a matter of being—it seems odd that sympathy with *others* should be included in it. Surely A does not become B when he sympathises with B. And if sympathy, as in its usual sense, is sympathy with somebody

else, it is complete nonsense to say, as M. Bergson does (p. 206), that assuredly we sympathise with ourselves.

In the fourth place, M. Bergson's view that a living thing is "conscient en droit" (p. 117) seems arbitrary. Granting that biological categories are less inappropriate to mind than inorganic ones, it does not follow that life and mind must be regarded as identical in principle. I hope that lettuces are not conscious, and I wonder what M. Bergson knows about their rights in this matter.

Suppose, however, that these objections are of little weight. What results does M. Bergson obtain from his metaphysics of inside information?

It is accepted on all hands that M. Bergson excels in delicacy of psychological observation, and that his descriptions of *durée* are a psychological achievement of the first order. It is further evident that, whether or not his account of sympathy with nature and with other men is adequate, his account of time that is lived or experienced has profound general significance for philosophy (even if Mr. Alexander is right in asserting that M. Bergson's services to philosophy would have been still greater had he also supplied an equally faithfully account of the space that is lived and experienced). Again, since M. Bergson's employment of his intuitional method in metaphysics is adventurous pioneer work, there is little ground for complaint in the circumstance, if circumstance it be, that he overworks his clues. That would be entirely legitimate in an experimental metaphysic, although it may be less admirable if it approaches dogmatism.

M. Bergson's essential clue is the contrast between the methods of external observation, conceptually supplemented, and the method of inner or immediate experience. Now the effective way of pointing a contrast is to develop the sharpest possible antithesis, and an experimental exaggeration of the opposition is a risk that is incident to the method. Again, since M. Bergson is prepared, in the end, to diminish the antithesis on his own terms—on the principle (p. 240) that selections may be extracted from an integrity, but that the extracts can never reconstitute the integrity—his merely antithetic method is not his last word. Nevertheless it seems necessary to enquire whether there is not exaggeration in what he actually says.

The full antithesis, as he states it, yields a truly enormous contrast. By the external method, he tells us, we obtain detached fragments that can only be juxtaposed; by the internal we obtain an indivisible continuity which is also a genuine whole. Externality gives us the mere repetition of identities, and in principle it is poor and exhaustible; the internal method gives us genuine and unpredictable novelty, inexhaustible in principle and truly "creative". Externality gives us mere passivity; the internal method reveals activity. And so the story goes on. It may not be obvious what sympathy and creativeness have to do with one another; but M. Bergson appears to think that if any way of knowing is not

based upon external observation, it must belong to the domain of intuition. *Tertium non datur.*

As we have seen, it may not be true that the external observation of change is either impossible or surreptitiously introduced by intuitional sympathy. Sensible continuity, again, is a plain characteristic of ordinary perception, and there is no lack of perceptual cues for an extension of perceptual limits that is *not* the juxtaposition of discontinuous entities. Here Nature seems to have blunted one of the blades of M. Bergson's scissors; and the other blade seems to have a certain bluntness in itself. For M. Bergson admits that there really is *succession* (p. 188)—although there is not *discontinuous* succession—and he also holds that time is a delayer (p. 118). It prevents everything being given at once (*ibid.*). His subtle analysis, therefore, of the way in which present experience employs and is avid of its past instead of simply coming after that past, is not the whole of the phenomenon; and surely there is *some* empirical and obvious sense in which the past *has passed*.

If observation, as we commonly say, "makes no difference" to the events concerned, we may, no doubt, call these events "passive" in their relation to the observer. It does not follow, however, that we cannot observe such events making a difference *to one another*, or that the selectiveness of perception denies its power to select an effective natural partnership. Leaving "activity", however, let us examine the novelty and creativeness for which intuition is said to vouch, and its alleged utter disparity from the mere repetition and reshuffling of changeless entities to which, according to M. Bergson, we should be condemned if we did not have intuition to tutor us.

M. Bergson claims that he knows by personal experience at every moment that there is an unpredictable novelty in his life at that moment (p. 115), and I suppose we should all admit that part of the very meaning of psychological presentness (and there may be no other kind of presentness) includes a certain freshness and vivacity (however jaded or bored we may be). Again, since the present has never occurred before, we may say, if we choose, that this particular freshness and vivacity is genuinely novel. But it is surely not novel in the sense that it is not similar to what we felt at *every* previous present; and if the novelty in question means radical dissimilarity between present and past, it is hard to see how any experience could vouch, of itself, for such dissimilarity. Again, this experience of freshness, or, in a word, of presentness, is not unpredictable; for M. Bergson, like everyone else, does predict it for all his future experiences; and his view (p. 156) that the unpredictable is "refractory to law and measurement" would appear to be the sort of principle that no immediate experience could conceivably attest. His further statement that *durée* is too rich (p. 35) to repeat itself, being always inexhaustible (p. 205), seems arbitrary, especially if (as on his theory) the doctrine must apply to all biology.

Why must there be an unpredictable richness in every pea, and a predictable poverty in all the oceans? Is it inconceivable that two peas (as in the proverb) are excellent examples of repetition and similarity, and the weather a very good example of capricious novelty? No doubt our scientists are becoming more successful in their predictions about the weather; but Mendelian scientists are also gaining confidence about the peas.

Indeed, granting that time and process are much more mysterious than the usual perfunctory accounts of them are disposed to admit—and he would be a bold man who denied this after pondering what M. Bergson has to say—it may reasonably be doubted whether phrases like “radical” or “creative” novelty are not also enigmatic in the extreme—despite M. Bergson’s art. “Le simple bon sens”, one might say, appears to indicate that there is novelty whenever anything objectively occurs that has not occurred before; and subjectively we reckon as novel whatever is surprising and unexpected. In the former sense there is plenty of room for novelty on a deterministic and, for that matter, on a mechanical system. For a new arrangement is quite authentically new. In the latter sense there is also plenty of room for the experience of surprise; for even what is predictable theoretically may very well be unexpected to most men, and would be unexpected to everyone if there were insufficient data for what, theoretically, was predictable.

The emphasis in M. Bergson’s theory, therefore, must be on *creative* novelty, and it does not seem to me to be easy to say what this conception means, however resourceful M. Bergson’s genius may be in describing the sort of biological or psychological events that M. Bergson considers creative.

In general when we describe a process as creative we mean that it is imaginative. The work of art comes out of the artist’s head—that is to say, out of nothing. In that case the conception of imagination needs to be revised; for the imagination cannot thus be finally separated from memory and sense-experience. And growing things do not grow out of nothing. If we say that their *newness* comes out of nothing, although the rest of them is not creative, we should appear to have an impossible dualism between their creative and their non-creative mode of being; and if life always implies *emergence*, such emergence may be a thoroughly repetitious affair; for it must include all the parallel emergences among the members of a flock of sheep.

JOHN LAIRD.

VI.—NEW BOOKS.

An Introduction to Logic and Scientific Method. By MORRIS R. COHEN and ERNEST NAGEL. New York: Harcourt, Brace & Co., 1934. Pp. xii + 467. \$2.75.

BEFORE 1930 no comprehensive Introduction to Logic written from the standpoint of modern developments of logical doctrines had been offered to the English reading public. Since that date four such Introductions have appeared, two in England and two in the United States. The fourth of these is the volume now being reviewed.¹ From the point of view of the teaching of elementary logic it is all to the good that four modern textbooks should be available. In many ways they supplement each other, what is worst done in one of them being better done in one of the others. It is much to be hoped that before long these books will supplant the traditional type of textbook still too much in use in the universities, thus giving new life to the teaching of logic to students entering for elementary examinations. Prof. Cohen and Dr. Nagel are to be congratulated upon having written a useful book.

The authors stress their conviction that 'logic is the autonomous science of the objective though formal conditions of valid inference' (p. iv), and are concerned to maintain this view 'against all attempts to confuse logic with psychology, where by the latter is meant the systematic study of how the mind works'. They seem to desire to censure writers on logic 'since the days of Locke' for having engaged in 'a good deal of speculative discussion as to the general nature of knowledge and the operations by which the human mind attains truth as to the external world' (p. 20), on the ground that such discussions 'are not necessary for the determination of any strictly logical issues'; nevertheless they find it necessary to examine three ways (or, as they say, 'methods') of arriving at beliefs, which 'are not free from human caprice and willfulness' (p. 195). Thus, notwithstanding their disclaimer, psychological considerations are not entirely excluded. A statement may be quoted from the Preface, which shows clearly the authors' intention: 'The present text aims to combine sound logical doctrine with sound pedagogy, and to provide illustrative material suggestive of the rôle of logic in every department of thought. A text that would find a place for the realistic formalism of Aristotle, the scientific penetration of Peirce, the pedagogical soundness of Dewey, and the mathematical rigour of Russell—this was the ideal constantly present to the authors of this book.' In conformity

¹ The other three, in order of publication are: L. S. Stebbing: *A Modern Introduction to Logic*; R. M. Eaton: *General Logic*; C. A. Mace: *The Principles of Logic*. A very elementary textbook, based upon Eaton's book, has been written by Chapman and Henle. All these books have been reviewed in MIND.

with this statement they stress the value of Aristotle's logical doctrines; of the traditional logic they say: 'Its chief drawbacks are less with what it has done and more with what it has failed to do' (p. 111). These defects their exposition seeks to remedy along lines which are by now familiar to all who are interested in the study of logic. It is not necessary, in a short review, to go through the chapters in detail; it must suffice to say that the usual topics are dealt with, always usefully, often interestingly. Their illustrative material is well chosen and admirably used; this is especially the case in Chapter VIII. ('Probable Inference') and Chapter XI. ('Hypotheses and Scientific Method').

The authors' treatment of induction and scientific method owes most to Peirce, although their terminology is mainly that of W. E. Johnson. They accept Peirce's theory of probability, namely, that 'the very meaning of probability involves *relative frequencies*'. This view is consistently applied and is shown to be plausible. From it there follows the consequence that general propositions can be established only by the method of repeated sampling. But it is not inconsistent with this view to admit that it is the ideal of science to achieve a systematic interconnection of facts. On the contrary: 'it is this systematic character of scientific theories which gives such high probabilities to the various individual propositions of a science' (p. 395).

Two further points require comment. The authors seem to be extremely anxious to insist upon the connection between logic and ontology, but it is not at all clear what is the point which they are concerned to make. 'Logic', they say, 'may be conceived as ruling out what is absolutely impossible, and thus determining the field of what is in the absence of empirical knowledge is abstractly possible' (p. 21). But how can the absence (or otherwise) of empirical knowledge affect what is 'abstractly possible'? The authors give no hint of how they would answer this question. They nevertheless make considerable use of the notion of possibility, especially with regard to the question how false propositions can 'have implications that are objectively necessary', and with regard to the nature of abstractions and limits. The discussion of the latter is, indeed, very unsatisfactory; so, too, is the discussion of fictions. Unclearness with regard to the notion of possibility is a serious defect, seeing how much weight is attached to it. It is surprising that 'possibility' does not appear in the index; the omission suggests that the authors have not realised how important, from their point of view, is the clarification of this notion.

The second point referred to above is the authors' attempt to exhibit scientific method in history, in moral judgements, and in art. These chapters are not of much value. The discussion of art is, in the opinion of the present reviewer, peculiarly inept. The authors appear to have confused 'art' with 'an art'. 'The rules of any art', they say, 'are more or less successful summaries of past experience in it' (p. 358). There are a few vague and rather superficial statements about aesthetic criticism, and a remarkably jejune discussion of poetry. The authors would have been wiser had they omitted this chapter.

There is an extremely useful Appendix in which is discussed the question: 'What does a Demonstration establish?' followed by some carefully analysed examples of fallacious demonstrations. Both this Appendix and Chapter VII. ('The Nature of a Logical or Mathematical System') will be of great use to the more advanced student.

At the end are given a number of exercises, arranged under the chapter

headings. Many of these exercises are new and will provide useful material for class teaching. There are a few misprints, for the most part unimportant, but a curious slip, on page 310, may puzzle the elementary student. In giving the mean deviations of a certain group, 5 has been written for 6; this slip has resulted in miscalculations in working out the example. It is somewhat unfortunate, too, that the word 'error' should have been used for 'deviation' without any etymological explanation; this usage is very puzzling to beginners, especially in the phrase 'probable error'. These are but small faults, however, in a book which will undoubtedly prove of great use to those engaged in teaching logic. It is to be hoped that the time is now approaching when writers of textbooks in logic will not need to think first of reforming the teachers.

L. SUSAN STEBBING.

The Nature of Mathematics. By MAX BLACK. London: Kegan Paul, Trench, Trubner & Co., Ltd. New York: Harcourt Brace & Co., 1933. Pp. xiv + 219. 10s. 6d. net.

THIS book is designed "to present a considered critical exposition of *Principia Mathematica*, and to give supplementary accounts of the formalist and intuitionist doctrines in sufficient detail to lighten the paths of all who may be provoked to read the original papers" (p. xiii) in which these doctrines are expounded. All technical terms and symbols have been as far as possible defined so that the book may be useful not only to specialists.

The critical exposition is very sketchy, and the accounts of the formalist and intuitionistic theories quite insufficient to throw more than a glimmer on these difficult paths. Nevertheless, I think the book will be useful, because Mr. Black has a very good nose for the most important points in the theories he is discussing, and whenever he believes that a difficulty has not been genuinely solved, he tells us plainly.

The introduction contains a short statement of the aim of philosophical analysis when applied to mathematics and a preliminary sketch of Logistic, Formalism and Intuitionism. Each of the theories should elucidate the notions (1) of 'integer' (2) of the mathematical continuum (p. 19). According to Logistic, mathematical propositions are propositions of logic; they state relations between propositions whose content has been abstracted to leave only their form. According to Formalism, mathematics is concerned with the structural properties of symbols (and hence of all objects) independent of their meaning. According to Intuitionism, mathematics is based on the intuition that to every collection extra members may be added. This intuition provides the series of natural numbers. General statements such as "There is a prime number the sum of whose digits is divisible by 1004" have meaning "only when a definite construction is known by which they might in theory (though not necessarily in practice) be tested for truth or falsehood with the certainty of obtaining an answer" (p. 10).

Section I.¹ (pp. 15-144) is an exposition of the logistic theory, and is concerned with five main topics.

(i) *Analysis*.—The discussion of this is confused and inadequately illustrated. At all times Mr. Black is willing to use the formula *Analysis*

¹ There is no Section II.

exhibits structure. But his efforts to further explain this statement are unsatisfactory.

(a) Applied to mathematics, analysis shows the interdependence of theorems, axioms and definitions and distinguishes between hypotheses and principles, etc., but does not eliminate contradictions (p. 4). (b) Analysis elucidates the notions denoted by symbols (pp. 19-23). (c) "Logical analysis is a method for elucidating the structure of systems of symbols" (p. 24); and finally, "*Logical Analysis of symbols consists in showing their logical form, that is their type, level, or multiplicity, more explicitly*" (p. 34). Whether these conflicting statements apply to different sorts of analysis Mr. Black does not say. And does he hold that logical analysis is concerned with the structure of symbols or with the structure of the facts for which they are used? Logical analysis in the sense of the analysis often practised by logicians is the reformulation of sentences with a view to exhibiting the structure of facts; and logical analysis in the sense of the analysis of logical and mathematical facts is designed to exhibit the structure of facts about classes of sentences. But neither of these points justifies Mr. Black's definition (c). His confusion between the structure of a symbol and that of the fact for which it is used is very manifest in the statement "Complexes of symbols . . . can function as elements, and by substitution in other complexes lead to the construction of symbols of ever-increasing complication. To some extent this complexity is visibly manifested in the visibly increasing number of the signs used, but to a great extent . . . this complexity is latent" (p. 25). The complexity which is visibly manifest is not the one which is latent.

Type, level and multiplicity are mixed up in a pretty scandalous way.

(ii) *The Calculus of Propositions.*—The description and illustration of this occupies only nine pages, and yet fulfils its purpose.

(iii) *Variable, Function, Description and Incomplete Symbol.*—Several different meanings of 'variable' and 'function' are distinguished. I believe that some of these distinctions are valuable, but the pace is far too hot for me.

It is now inexcusable to say that classes are incomplete symbols.¹

(iv) *The Disorders arising from the Application of Logistic to Continuity.*—Certain paradoxes seem, on the logistic theory, to require for their solution Russell's theory of types. Paradoxes of the same kinds appear in the mathematics of the continuum. Yet some of the theorems in this very branch of mathematics Russell's theory of types renders invalid.

(v) *The Cures prescribed by Ramsey, Weyl, Wittgenstein, Chwistek.*—The account of these is stimulating, but utterly insufficient.

Two supplements (pp. 147-210) are expositions of Formalism and Intuitionism—both designed to avoid the difficulties of Logistic. Enough is said about these theories to justify Mr. Black's claim that they deserve careful attention, but not enough to give people an understanding of them. As far as I can see, which is not far, there is a good possibility of reconciling them both with Logistic when we remember the sense in which mathematics is formal and the principle of acquaintance with the constituents of the judgments we make.

There is a bibliography.

This work is a series of notes rather than a book, though it is not ill-arranged. I repeat that it will serve a purpose because it (a) throws into relief present problems in the philosophy of mathematics, and (b) draws

¹ L. S. Stebbing, *A Modern Introduction to Logic*, p. 149.

attention to attempted solutions too long neglected in this country. It is to be hoped that Mr. Black will soon, but very slowly and rather elaborately, tell us more about these.

JOHN TERENCE WISDOM.

Scientific Theory and Religion. By ERNEST WILLIAM BARNES. Pp. xxiv + 685. Cambridge University Press, 1933. Price 25s. net.

It is impossible within the compass of a brief review to do adequate justice to Dr. Barnes's great book embodying his Gifford Lectures at Aberdeen in 1927-29. The sub-title is 'The World described by Science and its spiritual interpretation' and to the writing on this immense theme the author brings a singularly clear scientific mind and deep religious understanding and experience. The first sixteen chapters are almost exclusively in the form of an encyclopædia of scientific knowledge. Four of these chapters are devoted to a detailed survey of matter, the geometrical concepts of space and the theory of relativity. The electrical theory of matter, heat and light, and the quantum theory of the atom are next passed in review, followed by two chapters on astronomy. It must be said at once that, up to this point, the reader is evidently presumed to be a mathematician of the standing, at least, of a Wrangler. If he is not, he must be prepared to turn over many pages before reading, at intervals, sections in which the Bishop writes his reflections on the particular scientific theory under discussion and on its relation to the general theme of the book. For example, after a careful mathematical exposition of the general theory of relativity, the author writes as follows: 'Until comparatively recently there was a feeling, among those men of science who accepted the idea of the Divine ordering of the Universe, that we could see a manifestation of God's will in the great laws of physics. . . . Then came Einstein. He led us forward and upward till . . . we thought to gaze upon some new ocean which more fully should reflect the will of God. But all such hopes have been disappointed. It is true that from our new vantage ground we see more clearly; but, whereas we had thought to know less imperfectly the mind of God, we have apparently discovered that all the while we have been gazing into the mind of man.' Now, to the non-mathematical reader, the many pages of mathematical arguments preceding the passage quoted convey no meaning, although he may gain a little insight (tinged, no doubt, with awe) into what is known as mathematical proof; but he would be content with—and, certainly, more edified by—a non-technical exposition. This is only one of many instances where the Bishop's deep insight into, and his well-balanced expression of, the implications of modern scientific theory may be lost to the general reader because of the unpalatable form of the great part of the surrounding matter.

Chapters on 'the origin of life and the geological record', 'the evolution of plants and sex', 'the evolution of animals and Mendelism', 'the machinery of evolution' and 'man's origin and past' complete the scientific background. These chapters are clear and fascinating expositions of geological and biological science, and the general reader will feel under a deep obligation to Dr. Barnes for bringing together in compact and lucid form the chief results of scientific research in these fields.

Having completed his picture of scientific achievement, the Bishop then enters the realm of philosophy. He discusses in detail such questions as "How far do the concepts and theories of the scientist give us knowledge

of the 'real' world?", "Are matter and mind two disparate entities and, if so, how can they interact?" It should be said that his philosophic position is that of moderate realism and his fundamental assumption, which he proceeds to justify, is the existence of a Universal Mind, that is to say, God. Summing up at this stage, he writes: "Though all values are not excluded from the realm of scientific enquiry, the fact remains that good and evil find no place in scientific schemes; and therefore no arguments strictly based upon such schemes can lead to a God Whose goodness draws us to Himself. Yet unless we can think of God as a Father, responsive to our struggle to overcome evil and ready to aid us by some measure of communion with Himself, He is not the object of religious aspiration but merely the end of a limited range of speculative enquiry."

In a chapter on religious experience, he discusses 'prayer', 'conversion, its psychology and value', 'mystical experience', 'the problem of public worship', 'the sacraments'; and in a section on the revival of pagan sacramentalism he reiterates, in restrained language, his well-known views.

In the concluding chapter on Immortality, Dr. Barnes clearly states and elaborates his conviction that belief in God and belief in the survival of personality must in the long run stand or fall together. Notwithstanding the new relativistic ideas of space-time, the author is convinced that we cannot reach any satisfactory solution of the problem concerning the relation of God to time, frankly admitting that here we are in a region where our mental powers fail to shed any revealing light.

It is hardly likely that Dr. Barnes's arguments—especially in the last few chapters—will escape criticism in several quarters but, to the earnest seeker after the higher verities, the book will be a source of expert information and of genuine inspiration. It is given to very few men to think clearly, to see clearly and to write clearly; Dr. Barnes is undoubtedly of this select number; as the reader finishes the book, he is left with a real feeling of admiration for the author's wide erudition (which no critic can gainsay) and his deep and sincere convictions on the greatest theme with which the mind of man is challenged.

W. M. S.

La Pensée. Vol. I. By MAURICE BLONDEL. Paris: Alcan, 1934. Pp. xli, 421. 60 Fr.

THIS book is the first of two volumes that attempt to obtain a *sanatio in radice* for all philosophy by resolutely observing a devout fidelity towards nature, thought and God.

Even in French the word *pensée* suggests something like *cogitatio* (la pensée pensante), and hints at something rather like epistemology. Faced, however, with the question why he did not interpret *pensée* in this (apparently) natural sense, the author replies that his method forbade him. His theme demanded, not thinking only, but what is thought, and, furthermore, that immanent process in things by which Nature, however desubjectivised, presses on to life, consciousness, reason and divinity. Even in "matter" there is an immaterial intelligible essence; and the unity of what we call thinking with its "objects" confounds the current misconceptions of idealisms and of realisms by its native simplicity, and illustrates the implacable monism of reality, despite (or because of) the multiplicity of Becoming.

This author would give their due to the singularities of life and of con-

sciousness; and he describes some of these with force and precision. He never allows himself to forget the proviso, however, that "on ne morcelle pas la vie véritable de l'esprit". Such phrases as *natura ut ratio*, *ratio ut natura*, and *ratio ut ratio* he holds, are all true, and are all true together. Yet even *ratio ut ratio* is not the end of the story. There is a flavour of deity everywhere. Atheism, like dualism, is stupid inadvertence. "Il y a donc avant, pendant, après ce que nous atteignons par la réflexion, une présence constamment entr'aperçue mais qui ne prend point figure reconnaissable, de même que nous sentons derrière nous la réalité d'un ami silencieux et invisible" (p. 405).

This thesis is sustained with learning, eloquence and charm; and although 200 pages (plus xli pages of introduction) may seem rather scant measure for so extensive and so majestic a theme, there are 200 pages more of *excursus* (thirty-one of them, and the first very long). In short, there is a second train for the baggage; and the baggage is well worth conveying, for it contains much learning, some careful analysis, plentiful evidence of a wide acquaintance with the sciences, and sundry pleasant gibes directed against Messrs. Bergson and Lévy-Bruhl.

JOHN LAIRD.

The Death of Materialism. By WHATELY CARINGTON (W. Whately SMITH). London: Geo. Allen & Unwin Ltd., 1933. Pp. 256. 10s.

MR. CARINGTON believes that 'numerous as have been the attempts to refute Materialism for good and all, none have been conducted quite on the right lines' (p. 31). His purpose in writing this book is to show what the right lines are. These lines are, he thinks, indicated by 'the inevitable outcome of the tendencies of modern thought' (p. 31). These tendencies appear to be (1) the liability of classical laws of physics to break down; (2) the elimination of Things-in-themselves; (3) the recognition of the fundamental unity of consciousness. As may be expected, Mr. Carington makes considerable use of Eddington's discussion in *The Nature of the Physical World*. Readers familiar with this work will be able to guess the line of Mr. Carington's argument. To establish his second point he makes use of the notion of fictions, drawing mainly upon Vaihinger's *The Philosophy of As-If*. The third point is supposed to be established by reference to 'the observable facts of paragnosis'. His conclusion is that 'we must infer a Universal Consciousness of which individual consciousnesses are fragments, offshoots, local centres, or something of that kind' (p. 229), whilst 'the conception of an external world acting in consciousness must be replaced by that of mutations of consciousness as the only reality, of which some, as it happens, are conveniently regarded as external' (p. 246).

The style in which the book is written is deplorable. In his Preface Mr. Carington says that 'the discussion goes considerably deeper than it can be carried in the language of Talk to Tiny Tots'. Perhaps this remark should prepare the reader for the facetious jokes, hardly above the level of a school magazine, with which the discussion abounds. One wonders what class of readers Mr. Carington had in mind; he continually pulls himself up on the ground that to say more would carry the discussion 'too deep'. The result is that his argument is scrappy and his manner unpleasant. There can, however, be no doubt of the seriousness of his purpose or of the enthusiasm which led him to write this curious book.

L. SUSAN STEBBING.

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- Magistri Eckardi Opera Latina: I, Super Oratione Dominica*, edidit R. Klibansky, Leipzig, F. Meiner, 1934, pp. xvi + 17, M. 2.50.
- C. W. Hendel, *Jean-Jacques Rousseau Moraliste*, 2 vols., London, Oxford University Press, 1934, pp. ix + 316; 348, 25s.
- R. Vauquelin, *Les origines de la psychologie pédagogique de Rousseau à Kant*, Paris, F. Alcan, 1934, pp. 194, 20 fr.
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- B. Dunham, *A Study in Kant's Aesthetics: The Universal Validity of Aesthetic Judgments*, Lancaster, Pa., 1934, pp. xiii + 142.
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- K. Saunders, *The Ideals of East and West*, Cambridge University Press, 1934, xxiii + 246, 10s. 6d.
- V. Cavallo, *La Libertà Umana nella Filosofia Contemporanea*, Naples, F. Perrella, 1934, pp. 473, L. 40.
- G. Lehmann, *Die Ontologie der Gegenwart in ihren Grundgestalten*, Halle (Saale), M. Niemeyer, 1933, pp. 42.
- H. Delacroix, *Les grandes formes de la vie mentale*, Paris, F. Alcan, 1934, pp. 187, 10 fr.
- R. Vauquelin, *Les Aptitudes fonctionnelles et l'Éducation*, Paris, F. Alcan, pp. 306, 35 fr.
- M. Lengart, *Essai sur les conditions du progrès moral*, Paris, F. Alcan, 1934, pp. 167, 15 fr.
- J. F. Hecker, *Russian Sociology: A Contribution to the History of Sociological Thought and Theory*, London, Chapman & Hall, Ltd., 1934, pp. xvi + 313, 8s. 6d.
- Hu Shih, *The Chinese Renaissance*, (Haskell Lectures, 1933), Chicago, University of Chicago Press (Cambridge University Press), 1934, pp. xi + 110, 7s.
- Rabbi L. Jung, ed. by, *The Jewish Library: Third Series*, New York, The Jewish Library Publishing Co., 1934, pp. xiii + 510, \$ 2.50.
- A. Berney, *Friedrich der Grosse: Entwicklungs-geschichte eines Staates*, Tübingen, J. C. B. Mohr, 1934, pp. v + 363.

- G. Whitehead, *An Inquiry into Spiritualism*, London, John Bale, Sons, & Danielsson, Ltd., 1934, pp. vii + 466, 10s. 6d.
- G. N. Belknap, *A Guide to Reading in Aesthetics and Theory of Poetry* (Studies in College Teaching—Bulletin 5), Eugene, Oregon, University of Oregon, 1934, pp. 91, \$ 50.
- G. Richards, *The Chain of Life*, London, John Bale, Sons, & Danielsson, Ltd, 1934, pp. 215, 6s.
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- D. B. McLachlan, *Monadic Astronomy*, privately printed, 1934, pp. 33.
- M. C. Stopes, *Birth Control To-day: A Practical Handbook*, London, John Bale, Sons, & Danielsson, Ltd., 1934, pp. 237, 5s.
- R. Cornwall, *Dark Interval*, London, Methuen & Co., Ltd., 1934, pp. 279, 7s. 6d.
- Abstracts of Dissertations for the degree of Doctor of Philosophy*, Vol. VI, Oxford, Clarendon Press, 1934, pp. 303.

VII.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF PHILOSOPHY. xxx., 26. **A. C. Benjamin.** 'The Logic of Measurement.' [Tries to show that "when we do measure we must not claim for our results more than is justified by the actual character of the measurement process. More specifically we must not suppose either that we have gotten rid of quality or that we have constructed a genuinely representative image of quality."] **R. B. Winn.** 'Whitehead's Concept of Process: A few Critical Remarks.' [Struggles with the relations of 'process' to 'time', the exact meaning of 'actual' and the distinction between 'mere' and 'real' potentiality, but ultimately owns himself beaten by Whitehead's obscurity.] xxxi., 1. **R. Demos.** 'The Conception of Derivation in Epistemology.' [Classifies derivations as logical, biological and psychological and points out that "*a priori* and innate do not mean the same thing"; so "it is evident that an idea may be independent of experience in the logical sense, and still be learnt, that is, obtained through the route of experience, in the biological sense."] **F. J. E. Woodbridge.** 'The Universe of Light.' [Comments on Sir William Bragg's book with this title, which conclude "yes: the man who has eyes and brain sees the visible world, but that world quite evidently is not in his eye, or in his brain, or in his 'mind.' It is in the light."] This number announces a new editorial policy with regard to book reviews. Ten regular reviewers are to review books still more rapidly and shortly. It seems doubtful whether this will improve the quality of the reviews, but it may please publishers. xxxi., 2. **P. L. De Lary.** 'Perspectivity and Objectivity.' [Attacks "Lovejoy's belief that perspectivity involves the wholesale negation of the very possibility of knowledge", by "placing objectivity beyond the reach of any possible experience", and argues that "objectivity (invariance) may be found within experienced perspectives." "It is not objectivity but obliteration which begins where perspectivity ends. Genuine knowledge always takes place from some particular standpoint."] **C. C. Pratt.** 'Objectivity of Æsthetic Value'. [Attempts to establish the objectivity of æsthetic value by the verdict of 227 undergraduates upon the music of Brahms, Mendelssohn, Mozart and Tchaikowsky. They were required to describe the music in terms of the epithets 'stately', 'sprightly', 'wistful' and 'vigorous', and Brahms was voted 'stately' by 91 per cent., Mendelssohn 'sprightly' by 99 per cent., Mozart 'wistful' by 97 per cent. and Tchaikowsky 'vigorous' by 92½ per cent.] xxxi., 3. **C. J. Ducasse.** 'On the Attributes of Material Things.' [Concludes that "in including qualities as well as material things under the vague question-begging heading of 'things', and treating as definitive of all 'things' the independence of observation which is true only of material things, Professor Perry is himself guilty of the fallacy that he calls that of 'definition by initial predication' with which he justly taxes Berkeley when the latter does the exact opposite."] **G. A. De Laguna.**

'Appearance and Orientation.' [Argues that "orientation is an irreducible factor in all perception and reference to standpoint is essential to the apprehension of anything as objective". But there ensues "a certain indetermination in our knowledge" of objects, because "things really different must appear alike".] xxxi., 4. **G. T. Kalif.** 'The Metaphysical Basis of Induction.' ["The self, then, along with some of the issues that it involves, namely, personal identity, the identity of the object, and the identity of the past, the present, and the future, constitutes the metaphysical basis of induction." Contends also that "the non-mathematical use of the term 'probable' is as fundamental and as meaningful as it is thoroughly human, simply because it is grounded in qualitative knowledge." "To remember, to act, to anticipate: these three form the core of human life and the ultimate ground of reflection" and "cannot exist apart from the self."] **J. V. Mauzey.** Report of the Thirty-Third Annual Meeting of the Eastern Division of the American Philosophical Association. This appears to have been largely occupied with continuing the debate, started by Prof. Pratt, as to what, if anything, is meant by 'idealism'. xxxi., 5. **V. C. Aldrich.** 'New Epistemological Method.' [Based on the Common-sense Principle, which "reads thus: what *appears* to be the case in veridical perception is *really* the case, and perception is sometimes veridical."] **D. A. Piatt** reports on the Tenth Annual Meeting of the Pacific Division of the American Philosophical Association. xxxi., 6. **E. Nagel.** 'Verifiability, Truth, and Verification.' [Holds that "there is no reason to suppose any proposition is verified once-and-for-all in any immediate experience, without the tacit use of pre-existing knowledge transcending the immediately given. And there is reason to suppose that the truth of a proposition is not the same as a *hic et nunc* verification." But "science is a self-corrective process of attaining knowledge" in which there is "a continual appeal from facts to principles, and from principles to facts."] **P. Goodman.** 'Neo-Classicism, Platonism, and Romanticism.' [Dryden Johnson and Aristotle are taken as exemplifying the first method of criticism, Plato the second, and Ruskin and Trotsky the third.] xxxi., 7. The Papers by **C. Barrett,** 'The Objectivity of Mind,' **G. A. Wilson,** 'The Organic Unity of Philosophy,' and **J. S. Moore,** 'Idealism, Mentalistic and "speculative"', all figured in the idealism—realism debate mentioned above. xxxi., 8. **H. R. Smart,** 'Professor Pratt on Speculative Philosophy' and **J. B. Pratt,** 'Once More unto the Breach!' conclude the protracted debate on 'idealism'. Prof. Pratt opines that "the word mind has an essentially subjective meaning" and that the use of it "inaugurated by Hegel in his treatment of *Der objective Geist*, and also in his reference to Nature as in some sense Mind, seems to me one of the most harmful things that a philosopher has ever done to philosophy." xxxi., 9. **C. W. Hendel.** 'The Status of Mind in Reality.' [Thinks that we have a problem more difficult than Descartes, because we must determine the status of mind in regard to a realm which is not only characterised by physical space, but also by historical time, but seems satisfied with the formal claim to truth and rightness which is implied in cognitive and moral judgments.] **S. Hook.** 'What is Materialism?' [Thinks that even when the pragmatic ('operational') test has shown that philosophers have been *talking* nonsense, it is still necessary to inquire what they must have *meant*. Using this method the author discovers that "the proper philosophical opposite of idealism is not realism but materialism", and that idealism "means that in some form or other teleological reference to values is to be read back into the structure and function of

what is commonly regarded as non-spiritual". Ultimately the issue is between naturalism and supernaturalism.] xxxi., 10. **H. Jaffe**. 'Selection among Cosmic Images'. [A strong argument for recognising the selectiveness of the scientific worker. "Always there is choice for the scientist" . . . who "too is an artist." "Probable error is attached to any series of observations; no observation will give us the *true* value; and the mean reading can never be more than an approximation of the true value." So "the task of the scientist is endless, for no finite labour will reduce observational error to zero."] **E. S. Brightman** and **D. C. Williams**. 'The Self, Given and Implied—A Discussion [as to whether a personalistic idealist can be convicted of four errors denounced by Prof. Williams in his article on 'the Innocence of the Given' in the *Journal*, xxx., 23]. Prints also Abstracts of the Papers read at the Annual Meeting of the Western Division of the American Philosophical Association in March, 1934. xxxi., 11. **A. G. A. Balz**. 'Whitehead, Descartes, and the Bifurcation of Nature.' [Proclaims an intention to discuss "the famous chapter in the *Concept of Nature* (to which chapter I confine my attention)" which relegates to 'metaphysics' the relation of the knower to the known and makes 'bifurcation' a 'fallacy'. But Whitehead is found so difficult that a 'retreat to Descartes' is soon effected, and the bulk of the paper discusses how Descartes arrived at bifurcation. The conclusion is that not only is matter a problem, as Whitehead declares, but also the soul.]

REVUE NÉO-SCOLASTIQUE DE PHILOSOPHIE. Tome 36, 2me Série, No. 41. Février, 1934. A handsome volume of 546 pages of essays contributed in honour of Prof. M. de Wulf on the occasion of his completing forty years as occupant of his Chair at Louvain. It is hardly possible to do more here than give the list of the very varied contents. **L. Noël**. *L'œuvre de M. de Wulf*. [Accompanied by a full bibliography, list of distinctions received, etc.] **E. K. Rand**. *The Supposed Commentary of John the Scot on the 'Opuscula Sacra' of Boethius*. [In support of the writer's view that this work embodies actual glosses by Erigena, and that the immediate author is not Remigius of Auxerre.] **F. Sassen**. *L'enseignement scolastique à Rolduc au xii^e Siècle*. [An interesting discussion of the contents of an early catalogue of the library of this famous abbey and the evidence thus furnished that Rolduc was a centre of "humanism", under the influence, through the school of Liège, of Chartres.] **H. Forest**. *Le réalisme de Gilbert de la Porrée*. [On the doctrines of Gilbert's well-known commentary on the *de Heptadibus* of Boethius.] **J. de Ghellinck**. *L'histoire de "persona" et de "hypostasis" !* [Concerned specially with an anonymous *liber de diversitate nature et personæ* by an unknown partisan of Gilbert de la Porrée.] **R. M. Martin**. *L'immortalité de l'âme d'après Robert de Melun*. [The analysis of Robert's arguments is followed by the text of the relevant chapters from his *Sentences*.] **A. Masnovo**. *Una polemica di Guglielmo D'Auvergne*. [Deals with the pronounced 'Voluntarism' by which W. controverts Arabian 'naturalism'.] **G. B. Phelan**. *An unedited text of Robert Grosseteste on the Subject-matter of Theology*. **G. Théry**. *Thomas Gallus et Egile d'Assise*. **O. Lottin**. *L'identité de l'âme et de ses facultés pendant la première moitié du XIII^e siècle*. [Deals with the history of the problem from William of Auxerre to Albertus Magnus.] **M. Grabmann**. *Eine für Examinazweck abgefasste Questionsammlung der Pariser Artistenfakultät aus der ersten Hälfte des 13. Jahrhunderts*. [On the interesting light thrown by this MS. 'Cram-book' on the way in which *philosophia naturalis* was conceived at Paris immediately

before the rise of Thomism.] **P. Mandonnet.** *Albert le Grand et le 'Philosophia Pauperum'.* [The malevolent critic against whom Albert defends himself in the work improperly called by this title is Roger Bacon. The book was written at a time when Albert could properly speak of himself as having *Socii*, i.e., before he received his "master's degree" in Theology at Paris, and the long series of scientific writings which it resumes and concludes must therefore belong to this early period of his life, contrary to the view of some eminent scholars. It follows that the severe strictures of Robert Grosseteste on certain *moderni* who misrepresent Aristotle in order to make a good Catholic of him by finding a 'creation in time' in his works are meant as reproof of his own indiscreet disciple Roger.] **A. Mager.** *Der νοῦς παθητικός.* [The writer contends that Aristotle's 'perishable' νοῦς is neither the *intellectus possibilis* nor the *intellectus agens*, but the *intellectus in actu*, and holds that St. Thomas himself has led subsequent interpreters astray on this point.] **A. Mansion.** *La théorie aristotélicienne du temps chez les péripatéticiens médiévaux.* [A careful study of the ambiguities of Aristotle's famous definition of Time, and the new turn given to the doctrine by Averroes, which is shown to influence the treatment of the subject by both Albert and Thomas. The all-important point is that, in order to preserve the unicity of Time, Averroes, unlike Aristotle, insists that the perception of it depends on the consciousness of internal change within the percipient.] **A. Birkenmaler.** *Avicennas Vorrede zum 'Liber Sufficientiæ' und Roger Bacon.* [How came Roger to be so much more fully informed about Avicenna's *Philosophia Orientalis* than his contemporaries? The question is answered here in large part by the publication of a Latin version of the Prologue to A.'s *Liber Sufficientiæ*.] **E. Gilson.** *Sur quelques difficultés de l'illumination augustinienne.* [Deals with the lack of any coherent doctrine of the formation of concepts in Augustinianism.] **D. E. Sharp.** *Thomas of Sutton, O.P.* [An account of the main positions of this early English Thomist. The writer describes him as O.P. though I observe that another contributor to the volume speaks of him both as a 'Benedictine' and as an 'Augustine Friar.' What is the fact?] **E. Hocedex.** *Une question inédite de Pierre d'Auvergne sur l'individuation.* [The text of the *quæstio* is given; the view defended is that the *form* is the principle of individuation, but it is a mistake to regard the doctrine as original in P. He derived it from Godfrey of Fontaines.] **G. Wallerand.** *Henri Bate de Malines et saint Thomas d'Aquin.* [A very lively account of the numerous strictures on St. Thomas contained in the *Speculum* of this interesting 'conciliator' of Aristotle with Plato. It is interesting to find that Bate not only knew the Latin versions of the *Phædo* and *Meno*, but was aware of the translation of the *Parmenides* by William of Morbeke, though he had not actually seen it.] **J. Hoffmanns.** *La Table des divergences et innovations doctrinales de Godefried de Fontaines.* [Account, with text, of the synopsis of the *Quodlibeta* of G. contained in the Merton MS. No. 138.] **E. Longpré.** *Étude sur les répétitions de Duns Scot.* [Describes the contents of MS. 139 in the library of the Cathedral at Valencia and discusses the light thus thrown on the composition of Duns's lectures on the *Sentences*.] **H. Pelzer.** *Barthélemy de Bruges.* [A short account of the life of this eminent humanist and physician, and more particularly of the surviving reports of his *Sophismata* (discussions of problems in the lecture-hall).] **F. Van Steenberghen.** *Le mouvement des études médiévales.* [A rapid survey of the progress made towards the systematic and international study of mediæval history and thought in the past forty years.] *Chronique.*

Tome 37, 2^e Série, No. 42. Mai, 1934. **C. Lemaître.** *Bergsonisme et Métaphysique* (cont.). [Bergson employs the methods of empiricism with unparalleled skill, and is absolutely loyal to their results. The inevitable consequence is that the universe as he describes it is visibly devoid of internal self-sufficiency, it is contingent through and through, without any sufficient principle of either efficient or final causality.] **A. Forest.** *L'exigence idéaliste de la philosophie contemporaine.* [The idealism considered is that of French thinkers from the opening of the last century (Cousin, M. de Biran, etc.) to our own time. The inspiration of the whole movement is due to the determination to escape from 'psychologism' and to reach a real knowledge of a real world. In this respect French 'idealism' presents resemblances to German post-Kantian philosophy but has always been marked by a 'humanism' which is not so characteristically German. The question, however, is whether even this 'humanistic' idealism can really succeed in establishing a genuine connection between being and knowing.] **M. Blondel.** *Pour la philosophie intégrale.* [A defence of the author's views against the recent vigorous polemic of F. Steenberghen, who denies that there can properly be such a thing as a Christian philosophy any more than there can be a Christian art of strategy.] **H. Favre.** *La philosophie de Przywara : métaphysique de créature.* [I regret my inability even to summarise this account of the work of a thinker who is clearly as difficult as able. P. evidently writes a very personal and baffling German, and from unacquaintance with his own works, I find myself really unable to follow the exposition.] **D. E. Sharp.** *Thomas of Sutton.* [Continues, with copious quotations, the account of this author's views begun in the last No.] Reviews, Chronique, etc.

KANT-STUDIEN. Band xxxix., Heft 1. 1934. **H. Reiner.** *Das Kantische Sittengesetz im sittlichen Bewusstsein der Antike.* [Adduces from the ancients maxims that anticipate Kant's formula of the moral law as given in the second Critique, and shows that some of them embody the same apprehension of the essential universality of the Good and the same sense of man's dignity as consisting in the free or autonomous choice of the Good.] **R. Unger.** *Zu Goethes Weltanschauung.* [Comprehensive review of the literature called forth by the Goethe centenary (1932). The only general comment of the author is that this aspect of Goethe's work received dominant attention.] Reviews (including a survey of recent Hegel literature). Reports of conferences of Deutsche Philos. Gesell. and Deutsche Gesell. f. Psychol. Note on the recent excavations on the site of Plato's Academy. Appendix—a third classified bibliography of recent philosophical articles and books in the chief cultural languages. The death of Hans Vaihinger is announced, and his death-mask reproduced.

Band xxxix., Heft 2. 1934. **H. Rickert.** *Kennen und Erkennen.* [Protests against identification of knowledge with direct acquaintance, and thereby of truth with copying. The object of direct acquaintance is single; thought breaks it up into a multiplicity of meanings organised into propositions, whose truth cannot be interpreted as copying. Even the relation of a concept to a sensum cannot be that of copying. This analytic or individualising movement of thought does not, however, preclude a theory of the All, for it bears within itself a generalising phase.] **G. Krüger.** *Der Massstab der Kantischen Kritik.* [Since the metaphysical possibilities in which the first Critique culminates themselves need a vindication, the other two Critiques, in which this vindication is supplied,

furnish the canon of the first. For Kant the metaphysical is the teleological ethically conceived.] **B. v. Juhos.** *Praktische und physikalische Kausalität.* [Criticism of the definitions of causality given by members of the Viennese Circle (chiefly Schlick).] Reviews. Notices include a sketch of the philosophical position of Heinrich Maier (d. Nov. 28 1933), and three letters (1869-70) from E. von Hartmann to J. E. Erdmann. Appendix—a fourth classified bibliography of recent philosophical articles and books in the chief cultural languages. With this Heft is issued No. 33 of *Philosophische Vorträge—Dilthey und die deutsche Philosophie der Gegenwart*, by J. Stenzel.

ERKENNTNIS. Band 4 (*Annalen der Philosophie*, Band xii.), Heft 1. 1934. **H. Behmann.** *Sind die mathematischen Urteile analytisch oder synthetisch?* [Redefining analytic judgments as judgments that express logical laws (or the application of these) and arguing within the Frege-Russell absorption of mathematics into logic, shows that all the judgments of pure mathematics must be analytic.] **A. Penttilä** and **U. Saarnio.** *Einige grundlegende Tatsachen der Worttheorie nebst Bemerkungen über die sogenannten unvollständigen Symbole.* [First part of an essay on the definition of "word" and "language", prompted by Carnap's dictum that "philosophical, that is, logical, investigation is the analysis of language". Claims that the class "word" covers four different types of the logical hierarchy.] **J. F. Brown.** *A Methodological Consideration of the Problem of Psychometrics.* [The supposition that science consists in measuring, correlating the measurements and then finding a theory to account for the correlations is too simple, is indeed wrong. Law, or at any rate theory, is prior to measuring (for the instrument is itself a physical system obeying laws some knowledge of which is necessary for the understanding of the use of the instrument). Hence no science is purely inductive. Measurement in psychology (in the Weber-Fechner law, and conspicuously in mental testing) has been a failure because we are ignorant of the laws which alone can direct, correct, and validate it.] Reviews.

VIII.—NOTES.

CORRIGENDA

To the Article *Indication, Classes, Numbers, Validation*, by A. N. Whitehead, Vol. XLIII, N.S., No. 171.

Some misprints in the symbolism of my article must, I fear, make the whole unintelligible, apart from the following corrections. The proofs arrived from across the Atlantic on the day when I was overtaken by a dangerous illness which has incapacitated me for work during three months. Thus there has been no opportunity for author's proof-corrections. I am obliged to the courtesy of the Editor for the belated insertion of these corrigenda:—

- P. 286. Def. I, replace (ϕ) by $(\mathfrak{A}\phi)$.
 „ Def. II, replace 1st occurrence of $p \cup q$ by $p \subset q$.
 „ Def. III, replace $\text{Ec! } x \cup p$ by $\text{Ec! } x \subset p$.
 „ Def. IV, replace $\alpha \cup p$ by $\alpha \subset p$.
 „ Four lines below, replace $\text{Ec! } x \cup \text{Ec! } x$ by $\text{Ec! } x \subset \text{Ec! } x$.
 P. 287. Lines 2 and 4, replace $(\mathfrak{A}\gamma)$ by (γ) .
 „ Def. VIII, replace $(\mathfrak{A}\alpha)$ by (α) .
 „ Last line but one, replace α^{No} by 2^{No} .
 P. 291. Def. XXIII, replace $(\mathfrak{A}S)$ by (γS) .
 P. 292. Def. XXVII, end of 2nd line, replace xy by ϕy .
 „ Def. XXVIII, replace $(\mathfrak{A}c)$ by (γc) .
 „ „ „ replace $a \cup c$ by $a \subset c$.
 „ Def. XXIX, replace $\phi(1, 0)$ by $\phi(0, 0)$.
 „ „ „ replace $(u +_c m, v +_c 1)$ by $\psi(u +_c m, v +_c 1)$.
 P. 293. Def. XXXI, replace $(\mathfrak{A}x)$ by (γx) .
 „ Line 8, replace \cup by \subset .
 „ Def. XXXIV, replace $(\mathfrak{A}x)$ by (γx) .
 P. 294. Lines 1 and 5, replace ϕ by Q .

I can only repeat my regret that prolonged illness and the breadth of the Atlantic Ocean should have led to so many corrigenda.

A. N. WHITEHEAD.

Harvard University,
August 26, 1934.

MIND ASSOCIATION.

At the Annual General Meeting which was held on 6th July at Cardiff, with the President, Prof. J. W. Scott, in the Chair, it was agreed to hold next year's meeting at Bedford College, London, at the time of the Joint Session with the Aristotelian Society, and Professor L. S. Stebbing was elected President of the Association for 1935.

The meeting decided that a general Index of MIND for the ten years 1924-1933 should be printed in a forthcoming number. Prof. Field drew attention to the fact that Philosophy is omitted at present from the list of subjects which can be offered by candidates in the examination for Inspectorships of Taxes; and the Association agreed that the omission was regrettable.

INTERNATIONAL CONGRESS OF REFORMED CHURCHES.

THE Second International Congress of Reformed Churches (Calvinist) will be held at Amsterdam from the 23rd till the 26th of October. The first congress was held at London in May, 1932. In agreement with the international committee, appointed in London, the Congress will be organised by a Netherlands commission representing various reformed Churches. The congress is based on the agreement arrived at in London, *viz.*, the reformed Confessions of Faith of various countries, such as the Westminster Confession, the Thirty-nine Articles of the Church of England, the Confession of La Rochelle, the Netherlands Confession, the Heidelberg Catechism and the Articles of Dordrecht. Correspondence concerning the congress should be addressed to the President of the Executive Committee, Prof. Dr. V. H. Rutgers, 117 Lairessestraat, Amsterdam.

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